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THE KINEMATICS OF MACHINERY.

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LECTURE II.

We shall in the present Lecture examine in some detail a few of the results which can be obtained by treating mechanisms upon the plan which Reuleaux has proposed, and which is illustrated by his models; that is to say, by the analytical treatment of which we have already seen the general nature.

We have seen how kinematic chains are built up from pairs of elements and links. The pairing and the linkage renders the relative motions in the chain absolutely determinate, and the determinate relative motion exists equally whether or not any link of the chain be fixed relatively to the earth or to any portion of space that we choose to treat as stationary.

We have now to consider in more detail the effect of fixing one link of the chain. In practice, of course, one link is always fixed, or in other words, its motion relatively to the earth, to a locomotive or whatever it may be, is made zero. A chain with one link fixed is simply what we know as a mechanism.

In examining pairs of elements we saw that we could fix either element of the pair with lower pairs, the relative motions remaining unaltered; with the higher pairs the inversion gives us a totally different motion. We have seen also that we can fix any one link of a kinematic chain just as we can fix either element of a pair. We therefore can get as many mechanisms from any chain as it has links. From any such chain as Fig. 3, for instance, which has four links, we can get four mechanisms. The fact that a kinematic chain gives us as many mechanisms as it has links appears, looked at from this point of view, a mere matter of course. It has, however, never been hitherto distinctly recognized, so far as I know, and it can hardly be realized too distinctly, the consequences which result from it being most important, as we shall see. All that I shall attempt to do in this lecture will be to look at some of the mechanisms obtained from the particular chain just mentioned, and various modifications of it.

We have already noticed that the chain has four links. We see further that it is a chain in which all the motions are conplane, each of its four pairs being simply a cylinder pair, and the four cylinder pairs having parallel axes. It is so proportioned that by causing one link to swing, another one can be made to revolve. In order that we may refer more easily to the links, a letter is attached to each in the engraving.

For convenience sake we may also use

* Abstract of lectures delivered at South Kensington.

a short symbol for this chain (the one used by Reuleaux) namely, (C''_4).* The C_4 within brackets stands for the four cylinder pairs, the symbol for parallel being added to indicate their relative positions. This is the symbol for the *chain*, no link being fixed. To distinguish the four mechanisms formed from

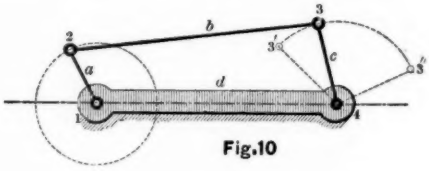


Fig.10

it, we shall put the letter which stands for the fixed link in the position of an index after the formula. Thus we can denote the particular mechanism shown in Fig. 10, in which the link d is fixed, by the formula $(C''_4)^d$. We have here then, the first of the four mechanisms we can get from this chain. You will recognize it easily enough as exactly similar to the beam and crank of a beam engine. The link c is half the beam, a the crank and b the connecting rod. The whole mechanism is an excellent illustration of what I said in my last lecture, that the *form* of the links is indifferent. If you think of the mechanism as forming part of a beam engine, for instance, you will see in the link d the abstract form of what is generally a most complex structure, a bed plate with its bearings, an entablature and plummer block, cast iron columns, and in some cases even brick and masonry. All these are represented by the fixed link d so far as their kinematic relations are concerned.

If now we fix the connecting rod b instead of fixing the link d as before, we have the mechanism $(C''_4)^b_{1r}$. It does not essentially differ from $(C''_4)^d$. The crank now revolves about the pin 2 which was formerly the crank pin, and the pin 1, which formerly represented the crank shaft, is now the crank pin, but there is nothing changed in the nature of the mechanism. By this inversion therefore we have got nothing new.

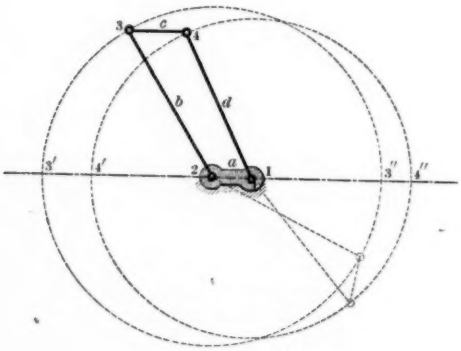


Fig.11

Let us now fix the link a, which was formerly the crank (Fig. 11). We have now the mechanism $(C''_4)^a$; it contains precisely the same elements as before, and the relative motions of the links are unaltered, but as a mechanism it is entirely different. It is now a combination frequently enough used in mills and elsewhere, known by the name of a "drag-link coupling." The links b and d have

* In words "C parallel 4."

become cranks, and one drives the other by means of the link *c*.

By fixing the remaining link of the chain, the link *c*, (which we have supposed to be longer than *a*), we have the entirely different mechanism $(C''_4)^a$, (Fig. 12). The two arms no longer revolve but only swing, and the link *a* turns right round once in every double swing of *d* and *b*. This mechanism is occasionally used in part of its stroke in parallel motions with some modifications, but is not so well known as the others.

The four inversions of this one chain, therefore, give us three different mechanisms. Looked at separately it is hard to see the relation in which these stand to each other; from the point of view which we have taken their mutual relationship has become at once evident.

in the direction of the axis of *d* or *b*. If no means be taken to prevent it, it is then possible to move the crank either in one direction or the other, and the two cranks may go on revolving in the same direction, or may revolve in opposite directions according to circumstances.

Such an indeterminateness is, of course, inadmissible in machinery, where we therefore adopt the well-known method of combining two mechanisms of the same kind, and placing them with their cranks at right angles, so that they do not cross the dead points at the same time. The motions are thus made determinate and the cranks revolve in similar directions. We might, however, wish them to revolve in *opposite* directions, as in the mechanism shown in Fig. 9. It

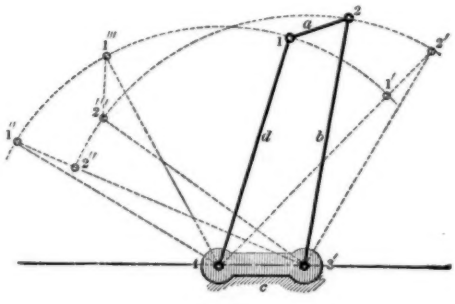


Fig.12

Without altering the pairing at all, we can greatly alter the chain by changing the relative length of its links. If we made all the links equal we should have a square, of which all four inversions would give similar mechanisms. If we make $b=d$ and $c=a$ we get a mechanism which is perfectly familiar in the couplings of locomotives and many other cases. All four mechanisms are again similar, each one consisting of a pair of cranks revolving with equal velocities and connected by a link which moves always parallel to itself.

These mechanisms are among those which have the peculiarity to which I alluded yesterday, that in one of their positions their motions are not determinate. This occurs at the "dead points" when *a* and *c* are both standing

may be worth our while to look for a moment at the means which may be used in this case to secure the determinateness of the motions in the mechanism. To distinguish between the two cases we may call the former "parallel cranks" and represent it by the formula $(C''_2 \| C''_3)$, and the latter "anti-parallel cranks," $(C''_2 \angle C''_3)$. This chain, with the link *d* fixed, is shown in Fig. 13.

In the case of the parallel cranks all points of the centroids of *b* and *d* are at infinity, for they are at the intersections of the parallel links *a* and *c*. We have already seen, however, that in the mechanism Fig. 9 the centroids are quite different, those of *b* and *d* being hyperbolæ. If, therefore, when the mechanism is brought into either dead point, where the cranks might change from the anti-

parallel to the parallel position, we can only make certain that the right centroids roll upon each other, we shall get the motion that we want. Fig. 13 shows an arrangement by which, just in that position of possible change, a tooth made on one link and a recess upon the other link gear together at a point corresponding to the point of contact of the centroids. The teeth G and H are virtually formed upon the centroid of *b*, and the recesses

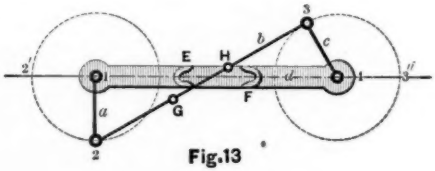


Fig.13

E and F upon that of *d*. At the points where these come into gear, the two centroids are compelled to roll upon one another, just as the pitch circles of two toothed wheels are compelled to roll on one another, and in this way the mechanism is carried over its only indeterminate point, and the cranks remain continuously anti-parallel and revolve in opposite directions.

This anti-parallel chain gives us two different mechanisms. Fig. 13 shows us $(C''_1 \gtrless C''_3)^d$. In the other mechanism $(C''_3 \gtrless C''_3)^a$ the two cranks revolve in the *same* direction with very varying velocity ratios.

Returning again to the chain (C''_4), it will be seen at once that we may substitute for the pair of elements at 4 a slot and a sector concentric with it, as in Fig. 14. The motions remain entirely unaltered. By adopting this contruction, however, it becomes possible to construct the mechanism without covering with it the center of the pair 4, *i. e.*, the point of intersection of the links *c* and *d*. We can therefore lengthen these links without making the mechanism inconveniently large. The only constructive alteration is that the slot becomes flatter as the links are lengthened. If we lengthen them little by little until they become infinitely long, the curved slot becomes straight, and its center line will pass through the point 1. The mechanism modified in this fashion takes the extremely familiar form already shown in Fig. 8. It now contains three cylinder pairs with parallel axes; the fourth cylinder pair has become a straight slot with a block working in it, namely, a prism pair. The axis of the prism pair is normal to the axis of the three cylinder pairs, and we may therefore use the symbol $(C''_3 P\perp)$ for the chain in its new form. There are here again four links, and therefore four inversions, and we shall find that all four mechanisms are now different.

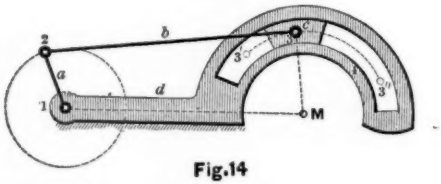


Fig.14

We have first the mechanism shown in Fig. 8, and familiar by its continual use in direct-acting engines $(C''_3 P\perp)^d$. Next, following the same order as before, we may fix the link *b*, the connecting rod of Fig. 8. The mechanism thus obtained, $(C'' P\perp)^b_2$, is quite different from the former, but equally familiar (Fig. 15). To make it more recognizible, the prism pair 4 is *reversed* in the figure, that is the link *c* is made to carry the open prism and *d* the full one. The motion is obviously unaffected by the change. The mechanism can be easily seen to be that

of the oscillating engine. The link c corresponds to the cylinder, swinging on fixed trunnions at 3, and the link d to the piston rod and piston of the steam engine. We see, then, that the relation between the mechanisms which are familiar to us as the driving trains of the have already seen This train has some practical applications in machinery, but is not very often used.

Here, then, we have obtained from one and the same chain four entirely different mechanisms, all of them more or less familiar. The method we have

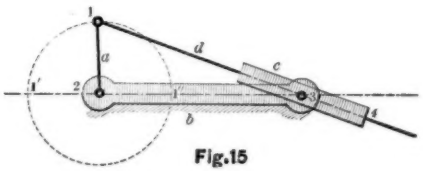


Fig.15

direct-acting and oscillating engines, is simply that they are different inversions of one and the same chain.

Let us now suppose the chain fixed upon the link which was the crank in the last two mechanisms (Fig. 16). This gives us a third mechanism which entirely differs from either of the two former

adopted has again been successful in making the real relations of these apparently dissimilar things perfectly obvious.

We have seen in connection with Fig. 14 that we can to a certain extent alter the size and extent of a pair of elements without altering its nature or changing

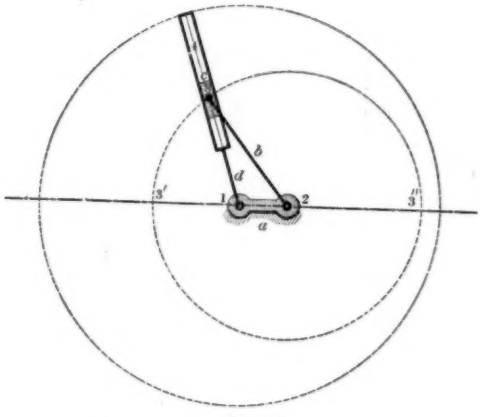


Fig.16

ones. It is quite familiar as a "quick-return" motion in some machine tools, for which purpose also the mechanism last mentioned has sometimes been used.

Fixing, lastly, the link c, we get the less familiar mechanism shown in Fig. 17 $(C''_1 P_\perp)^c$. The link b swings about 3, and the crank a rotates in space somewhat as in the mechanism $(C''_1)^e$, which we

the motion of the chain to which it belongs. This alteration in the size of elements, or what may be called the "expansion" of elements, is a process continually carried out by engineers for practical constructive reasons; and often gives to identical mechanisms extremely different forms. It is impossible here to go into this in detail, a somewhat extreme case of it is shown, for the sake of

illustration, in Fig. 18. Here we have the mechanism shown already in Fig. 8, $(C''_2P\perp)^d$. The pin of the pair 2 is so enlarged as to include altogether the pair 3, the connecting-rod *b* being simply a circular disc, with an eccentric cylindric hole in it. The pin 1 again (the "crank shaft"), is made large enough to include the whole of 2. We have therefore 3 within 2, and 2 within 1. We have one very common illustration of the extent to which this expansion of pairs is carried practically in the link motion. The curved link and block are in their kinematic relations simply a very much expanded pair of cylindric elements reduced in extent by use of a process similar to that by which we got Fig. 14 from Fig. 10.

We have seen what results have been obtained by making two links of the chain (C''_4) infinitely long. The same process can be carried still further. In the familiar chain shown in Fig. 7, for instance, *three* links, *d*, *c* and *b*, are made infinite. We have therefore another prism pair in it, and its formula becomes $(C''_2P\perp_3)$. It gives us the two mechanisms already mentioned and a third one, all of which are practically applied.

We must pass over without mention many other modifications and alterations of the chain, and mention only one other form in which it occurs, a form which has some special interest. The condition of movability of a chain that contains four cylinder pairs is not that their axes should be parallel, but that they should *meet in one point*. The axes are parallel only in the special case where this point is at an infinite distance. Fig. 19 is an illustration of the more general, although less familiar, case when the point of intersection is at a finite distance. This chain, which may be indicated by the formula $(C^{\perp}_4)$* has again its four inversions, and furnishes us with three different mechanisms as in the case of (C''_4). I cannot here go into these; I mention the chain partly because of its theoretic interest, and partly because—although it looks so unfamiliar,

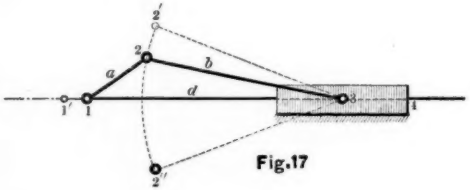


Fig.17

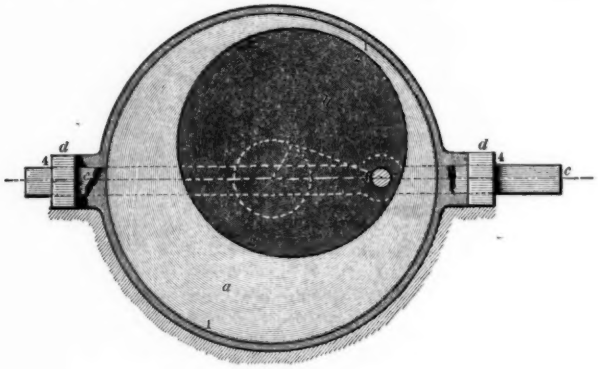


Fig.18

* "C four oblique."

it is not unfrequently applied in machinery. If instead of subtending only a small angle, as in Fig. 19, three of the links were made to subtend an angle of 90°, we should have the common Hooke's or universal joint. In these "conic" chains, links which are quadrants take the place of the infinite links, so that a chain having three quadrants corresponds to the chain of Fig. 7, in which there are three infinite links. The formula of the universal joint is $(C^1_3 C^1)^a$; it corresponds exactly to $(C''_2 P^1_2)^a$ and is analogous to $(C''_4)^a$ and $(C'^1_3 P^1)^a$, Figs. 11 and 16 respectively. I have already mentioned in passing that the mechanism $(C'_3 P_2{}^1)^a$ is that of Oldham's coupling. We see then clearly the close relationship in which this coupling (for parallel shafts) stands to the Hooke's joint or coupling for inclined shafts. It is another illustration of the important results which follow naturally from Reuleaux's simple method of analysis, and which hardly seem attainable, certainly not with equal directness, by any other method.

We may notice one other way in which the chain (C''_4), which we have seen in so many forms, may be modified. In some cases we may not wish to utilize the motion of all the four links, but (say) only three of them; in that case we may omit the fourth link, if we carry out a proper pairing between the two links thus left unconnected. If, for instance,

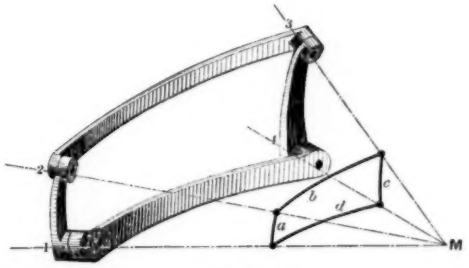


Fig.19

we omit the link b we must make a proper pairing between a and d. This pairing will always be higher; the chain becomes a *reduced* chain. Such a chain, reduced by b, is shown in Fig. 20. The higher pairing is carried out by placing upon a a suitable element, here a circular pin at 2, and giving to c the form of the *envelope* of the motion of the pin relatively to it. This envelope is the curved slot shown in the figure. It was not necessary to take the new element at 2, but if it had been in any other place, as 2′, the form of the slot would have been more complex, as is shown by dotted lines. For such a chain we may use the formula $(C'_3 P^1) - b$. The process of reduction can be carried on in this way until only two links are left,

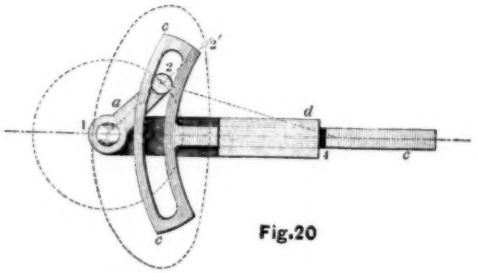


Fig.20

which then become really a pair of higher elements. It is constantly employed in machinery, mostly in the case of compound chains, or chains in which some links contain more than two elements.

The chain which we have been examining has been applied more often than any other to the leading trains of engines and pumps. We shall in conclusion look at a few of these machines, in order to notice the constructive disguises which often appear in them and render their kinematic identity almost unrecognizable.

Fig. 21 shows a rotary engine which

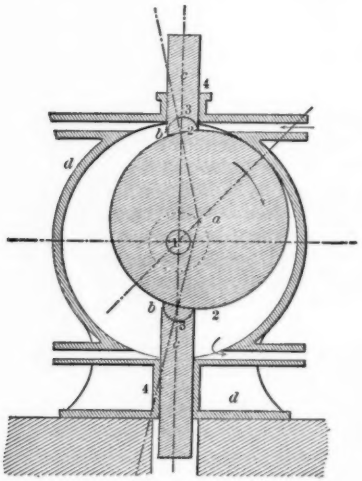


Fig.21

has been patented several times, and which is founded on the same mechanism, $(C''_3 P\perp)^d$, as the common direct-acting engine. The letters and figures placed upon it correspond to those on Fig. 8, so that the identity of the mechanisms may be the more easily traced. The extraordinary change of form undergone by the connecting rod b is worth special notice. It has become a bar having a cross section like a half moon. It still consists however, kinematically, of two cylinders or portions of them, one described about the axis of 2 (the center of the disc), the other about the axis of 3. The element 2 is expanded so as to include 1, the crank a becoming therefore a disc. The mechanism is so proportioned that the virtual length of the connecting rod—the distance, that is, between the centers of the elements 2 and 3, is equal to the radius of the disc a. With the mechanism in the form shown in Fig. 21 some separate means has to be provided for keeping b against a when the latter is moving upwards.

Fig. 22 is another rotary engine, which has been patented a dozen times since its first invention in 1805. It is based upon the mechanism of Fig. 16 $(C''_3 P\perp)^a$. The fixed link a is here made the steam cylinder, while d becomes a moving piston. The reference letters are the same as before. It will be noticed that the

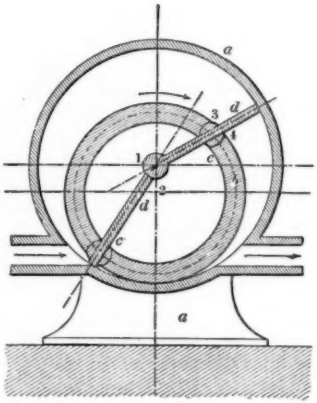


Fig.22

cylindric element of the link c is expanded to include its prismatic element; in all the cases formerly noticed the latter had been the larger.

In order to illustrate this part of his subject Reauleux examines (in the work I have already mentioned) some forty or fifty rotary engines and pumps all derived from the (C''_4) chain and such modifications of it as we have been looking at. Models of a number of these are now on the table before you. Many of them bear scarcely any external resemblance to the common steam engine, to which they are, notwithstanding their dissimilarity, so closely related. We are not now concerned as to which form is absolutely the best, but are only looking at them from a kinematic point of view. But it is worth noticing that in very

many cases not only constructive but mechanical advantages have been claimed for them. Their inventors have over and over again claimed some mechanical gain more or less mysterious, compared with the ordinary form of engine. Most of them, moreover, have been called "rotary" to distinguish them from reciprocating engines. Our method of analysis, although only kinematic, has shown us not only that there can be no mechanical advantage possessed by one over the other, but that the word "rotary" is essentially a misnomer, if it be supposed to indicate that there is any more or different rotation in them than in ordinary engines.

We shall now only notice one more of these engines. It is one which has puzzled people a great deal, and with very good reason, for its motions are very strange, and its analysis apparently very complex.

The engine I refer to is shown (in one form) in Fig. 23. It is known as the "disc engine." It was brought a good deal into notice in 1851, and for a few years was used in the *Times* office without any ultimate success. Analysis shows that it is based upon the same chain as the Hooke's joint, the conic chain, namely, in which three out of four links subtend right angles, and of which the formula is $(C^{1}{}_{3}C^{1})^{a}$. The fixed link is, however, *d*, while in Hooke's joint *a* (the acute-angled link) is fixed. Patents have been taken out also for disc engines in which *a*, and others in which *b*, is the fixed link.

I may say, in conclusion, that while it may be impossible for many educational institutions in this country to possess themselves of models so perfect in execution, and therefore so expensive, as those which have been sent to the Loan Collection, it is yet quite practicable to construct many of them in a form which, while quite cheap, is yet well adopted for educational purposes. I have some made in this way of hard wood with brass pins, which are very serviceable for college purposes. It is just the simpler models, which are the most easily made, which are the most useful in instruction.

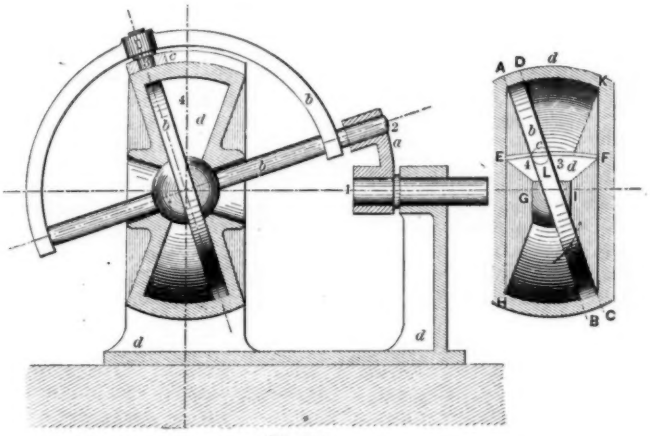


Fig. 23

I venture to hope that the treatment of the theory of mechanism illustrated by the models now in the South Kensington Collection, and of which I have here endeavored to set forth some leading principles, may prove valuable in aiding the study and the comprehension of this branch of machine science both to the student and the engineer.

A RECENT English invention for a boring machine consists of a compound slide in the form of an inclined plane, the incline being in the direction of the length of the boring bar. By moving one part of the rest on the other the work is adjusted vertically.

MOLECULAR MAGNETISM.

By PROF. D. E. HUGHES, F.R.S.

From "English Mechanic and World of Science."

DURING the course of some late researches, which I had the honor to communicate to the Royal Society and experimentally illustrate on the reading of my second paper so many experimental facts occurred, all pointing to the conclusion that ordinary molar magnetism is entirely due to the symmetrical arrangement of its polarized molecules, and that these molecules can be rotated by torsion, so as to decrease its longitudinal magnetism, or increase it if the effect of the elastic torsion is to rotate the molecules in its required longitudinal symmetrical arrangement, and observing that molecular magnetism could induce an electric current upon its own molar constituents, or that an electric current by its passage through an iron wire would produce molecular magnetism, I have continued these researches in the hope of elucidating, as far as possible, the phenomenon of the transformation of electricity and magnetism by the changes produced in the molecular structure of its conducting wire.

For this purpose I have employed three separate methods of investigation, each requiring a slightly modified form of apparatus. The first relates to the influence of an elastic torsion upon a magnetic or conducting wire; the second the influence upon the molecular structure of an iron wire by electricity or magnetism; the third was the evident movement of the molecules themselves as given out in sonorous vibrations. The general details of the apparatus employed having been given, I will only briefly indicate any modification of the method employed.

1. INFLUENCE OF AN ELASTIC TORSION UPON A MAGNETIC OR AN ELECTRIC CONDUCTING WIRE.

In the paper just referred to I showed that induced currents of electricity would be induced in an iron wire placed on the axis of a coil through which intermittent currents were passing, and that these currents were produced only when the wire was under the influence of a torsion not passing its limit of elasticity. It became evident that if the intermittent magnetism induced by the coil produced under torsion intermittent currents of electricity, that an intermittent torsion under the influence of a constant current of electricity or a constant magnetic field would produce similar currents. This was found to be the case, and as some new phenomena presented themselves indicating clearly the molecular nature of the actions, I will describe a few of them directly relating to the subject of this paper. An iron wire of 20 centims. was placed in the center or axis of a coil of silk-covered copper wire, the exterior diameter of the coil being $5\frac{1}{2}$ centims., having an anterior vacant circular space of $3\frac{1}{2}$ centims. The iron wire is fastened to a support at one end, the other passing through a guide, to keep it parallel but free, so that any required torsion may be given to the wire by means of a connecting arm or index. A sensitive telephone is in direct communication with the coil, or a galvanometer may be used, as the current obtained by a slow elastic torsion are slow and strong enough to be seen on a very ordinary galvanometer. I prefer, however, the telephone, because it has the inestimable advantage in these experiments of giving the exact time of the commencement or finish of an electric current. It has, however, the disadvantage of not indicating the force or direction of the current; but by means of the sonometer the true value and direction of any current is at once given. Again, the telephone is useless for currents of slow intermittence, but by joining to it the microphonic rheostat described in a previous paper a slowly intermittent or permanent current is broken up into rapid intermittent currents, and then we are able to perceive feeble constant currents. For this reason a microphonic rheostat is joined to the telephone and coil. The current from a battery of two bichromate cells is

*A paper read at the Royal Society, on May 19.

sent constantly through the wire if we wish to observe the influence of the torsion of the wire upon the electric current, or a constant field of magnetic energy is given to the wire by either a separate coil or a permanent magnet. The currents obtained in the coil are induced from the change in the molecular magnetism of the wire, but we may equally obtain these currents on the wire itself without any coil by joining the telephone and rheotome direct to the wire; in the latter case it is preferable to join the wire to the primary of a small induction coil, and the telephone and rheotome upon the secondary, as then the rheotome does not interrupt the constant electric current passing through the wire. As the results are identical, I prefer to place the telephone on the coil first named, as the tones are louder and entirely free from errors of experimentation.

If we place a copper wire in the axis of the coil we produce no effect by torsion, either when under the influence of a constant magnetic field or a current passing through it, nor do we perceive any effects if we place an iron wire (2 millims. in diameter), entirely free from magnetism and through which an electric current has never passed. I mention this negative experiment in order to prove that all the effects I shall mention are obtained only through the magnetism of the wire. If now I pass an electric current for an instant through this same wire, its molecules are instantly polarized, and I have never yet been able to restore the wire to its original condition, and the magnetization induced by the passage of a current is far more powerful and more persistent in soft iron than tempered steel. This may be due, however, to the fact that in tempered or softened steel we find traces only of a current during the rotation by torsion of its molecules some two to three degrees of sonometer, whilst iron gives constantly a current of 70 sonometric degrees.*

In order to obtain these currents, we must give a slight torsion of 5° or 10° to and fro between its zero point. We then have a current during the motion of the index to the right, and a contrary current in moving the index to the left. If we use a galvanometer, we must time these movements with the oscillations of the needle; but with the telephone it gives out continuous sounds for either movement, the interruptions being only those caused by the rheotome. The direction of the current has no influence on the result; either positive to the free arm or index or negative gives equal sounds, but at the moment of reversal of the current a peculiar loud click is heard, due to the rapid change or rotation of the polarization of its molecules, and this peculiarly loud momentary click is heard equally as well in steel as in iron, proving that it is equally polarized by the current, but that its molecular rigidity prevents rotation by torsion. We can imitate in some degree the rigidity of steel by giving the iron wire several permanent twists. The current due to elastic torsion is then reduced from 70° to 40°, due to the mechanical strain of the twists remaining a constant; and a weakening of the current is also remarked if with a fresh wire we pass in torsion its limit of elasticity.

If a new soft iron wire of 2 millims. (giving no traces of a current by torsion) has passed through it a momentary current of electricity, and then wire observed free from the current itself, it will be found to be almost as strongly polarized as when the current was constantly on, giving by torsion a constant of 50 sonometric degrees. If, instead of passing a current through this new wire, I magnetize it strongly by a permanent magnet or coil, the longitudinal magnetism gives also 70° of current for the first torsion, but weakens rapidly, so that in a few contrary torsions only traces of a current remain, and we find also its longitudinal magnetism almost entirely dissipated. Thus there is this remarkable difference, and it is that whilst it is almost impossible to free the wire from the influence produced by a current, the longitudinal magnetism yields at once to a few torsions. We may, however, transform the ring or transversal magnetism into longitudinal magnetism by strongly magnetizing the wire after a current has passed through it; this has had the effect of rotating the whole of the molecules, and they are all now symmetrical with longitudinal magnetism, then by a few torsions the wire is almost as free as a new wire; and I have found

* 0.8 of a Daniell battery.

this method more efficacious than heating the wire red hot, or any other method yet tried. If I desire a constant current from longitudinal magnetism, I place at one of the extremities of the wire a large permanent magnet, whose sustaining power is 5 kilometers, and this keeps the wire constantly charged, resembling in some respects the effects of a constant current. The molecular magnetism or the current obtained by torsion is not so powerful from this, my strongest magnet, as that produced by the simple passage of a current, being only 50 sonometric degrees in place of 70° for that due to the passage of a current. The mere twisting of a longitudinal magnet, without regard to the rotation of its molecule having no effect, is proved by giving torsion to a steel wire strongly magnetized, when only traces of a current will be seen, perhaps one or two degrees, and a constant source of magnetism or electricity then giving no measureable effect. Evidently we have equally twisted the magnetized steel as the soft iron. In the steel we have a powerful magnet, in the soft iron a very feeble one; still the molecular rotation in iron produces powerful currents to the almost absolute zero of tempered steel.

If we magnetize the wire whilst the current is passing, and keep the wire constantly charged with both magnetism and electricity, the currents are at once diminished from 70° to 30°. We have here two distinct magnetic polarizations at right angles to each other, and no matter what pole of the magnet or of the current the effect is greatly diminished; the rotation of the two polarities would now require a far greater arc than previously. The importance of this experiment cannot as yet be appreciated until we learn of the great molecular change which has really occurred, and which we observe here by simply diminished effects.

If we heat the wire with a spirit flame, we find the sounds increase rapidly from 70 to 90, being the maximum slightly below red heat. I have already remarked in my previous paper this increased molecular activity due to heat, and its effects will be more clearly demonstrated when we deal with the sounds produced by intermittent currents.

Another method, by means of which I have again received proofs of the rotation of the polarized molecule, is to pass an intermittent currency through a soft 0.5 millim. iron wire, listening to the results by the telephone joined direct and alone to the coil. If, then, the wire is entirely free from strain, we have silence, but a torsion of 20° produces some 50 sonometric degrees of electric force. If, now (the wire being at zero strain), I bring one pole of the permanent magnet I have already described near the side of the wire, the sounds increase from zero up to 50°, being at their maximum when this magnet is 5 centims. distant; but if we continue to approach the magnet the sounds gradually weaken to an almost zero. The explanation of this fact can be found when we know that the greatest inductive effect on the wire would be when a magnet is at an angle of 45° with the wire. And, also, considering each molecule as a separate independent magnet, we find that at a given distance for a given magnet the force of rotation is equal to that of 45°; by approaching the magnet we increase the rotation but diminish the angular polarity in relation to the wire, hence the decrease of force by the near approach of the magnet. And to prove that the function of the elastic torsion is simply to rotate the polarized molecules similarly to the magnet, we place the wire under an elastic torsion of 20°, and approach gradually the magnet as before. One pole now will be found to increase the sounds or its angular polarity, the other will decrease until at 5 centims. distance as before we have perfect silence; the torsion exists as before, but the molecules are no longer at the same angle. On removing the magnet we find that instead of the usual 50 of current we obtain barely 5 or 10; have we then destroyed the polarity of the molecules, or do they find a certain resistance to their free rotation to their usual place? To solve this question we have only to shake or give the wire a slight mechanical vibration, and then instantly the molecules rotate more freely, and we at once find our original current of 50°. I will forbear mentioning many other experimental proofs of my views by this method, as there are many to relate by different methods in the following chapters.

2. INFLUENCE UPON THE MOLECULAR STRUCTURE OF AN IRON OR STEEL WIRE BY ELECTRICITY OR MAGNETISM.

Being desirous to modify the apparatus already described, so that it would only give indications of a current if they were of a spiral nature, the wire was kept rigidly at its zero of strain or torsion, and the coil was made so that it could revolve on an axis pependicular to the wire; by this means, if the wire was free from strain, the center or axis of the coil would coincide with that of the wire. Thus with a straight copper wire, we should have a complete zero, but if this wire formed a right or left-handed helix, the coil would require moving through a given degree (on an arbitrary scale) corresponding to the diameter and closeness of the spirals in the helix; the degrees through which the coil moved, were calibrated in reference to known copper helices. 50° equaled a copper wire 1 millim. diameter, formed into a helix of 1 centim. diameter, whose spiral turns were separated 1 centim. apart.

In order to obtain a perfect zero and wide readings, with small angular movement of the coil, it is necessary that the return wire should be of copper, 2 millims. diameter, offering comparatively little resistance, and that it should be perfectly parallel with the steel or iron wire. In order that it may react upon the exterior of the coil, it is fastened to the board, so that it is near (1 centim.) the exterior of the coil, and parallel to the iron wire, at a distance of 4 centims. If we consider this return wire alone, we find, as in the sonometer, that if the wire is perpendicular to the exterior wires of the coil, we have a zero or silence, but moved through any degree, we have a current proportionable to that degree; by this means we have an independent constant acting on the coil, constantly aiding the coil in finding its true zero, and allowing of very wide readings, with a comparatively small angular movement of the coil.

The rheotome is joined to a battery of two bichromate cells, and by means of a reversing switch an intermittent current of either direction can be sent through the wire. The telephone is joined direct and alone to the coil, thus no currents react upon the coil when perpendicular to the iron, and its return wire, if not of a spiral nature.

Placing an iron wire 0.5 diameter, and passing a current through it, I found a change had taken place similar to those indicated in my second paper, but it was so difficult to keep the wire free from magnetism and slight molecular strains, that I preferred and used only in the following experiments tempered steel wire (knitting needles I found most useful). All the effects are greatly augmented by the use of iron wire, but its molecular elasticity is so great that we cannot preserve the same zero of reading for a few seconds together, whilst with steel, 0.5 millim. diameter, the effects remained a constant until we removed the cause.

I have not as yet been able to obtain a steel wire entirely free from magnetism, and as magnetism in steel has a remarkable power over the direction of the spiral currents, I will first consider those in which I found only traces. On passing the intermittent current through these, the sounds were excessively feeble for either polarity of current, but at each reversal a single loud click could be heard, showing the instant reversal of the molecular polarity. The degree of coil indicating the twist or spirality of the current was 5° on each side of its true zero. The wire was now carefully magnetized, giving 10° on each side for different currents. The positive entering at north pole indcating 10° right-handed spiral, negative entering the same pole, a left-handed spiral, we here see in another form, a fact well known and demonstrated by De la Rive by a different method, that an electric current travels in spirals around a longitudinal magnet, and that the direction of this spiral is entirely due to which pole of an electric current enters the north or south pole. I propose soon, however, to show that under certain conditions these effects are entirely reversed.

If through this magnetized wire I pass a constant current of two bichromate cells, and at the same time an intermittent one, the spiral is increased to 15°, but the direction of the intermittent current entirely depends on that of the constant current; thus, if the positive of the constant current enters the north pole, the intermittent positive slightly in-

creases the spiral to 17°, and the negative to 13°, both being right-handed; the two zeros of the constant battery are, however, as we might expect from the preceding experiment, on equal opposite sides of the true zero; but if we magnetize the wire whilst a constant current is passing through it, a very great molecular disturbance takes place; loud sounds are heard in the telephone, and it requires for each current a movement of the coil of 40°, or a total for the two currents of 80°. This, however, is not the only change that has taken place, as we now find that both constant currents have a right-handed spiral; the positive under which it was magnetized a right-handed spiral of 95°; the negative, a right handed spiral of 15°, and the true central or zero point of the true currents indicates a permanent spiral of 55°.

This wire was magnetized in the usual way, by drawing the north pole of my magnet from the center to one extremity, the south from the center to the other, and this repeated until its maximum effects were obtained; in this state I found, sliding the coil to different portions, that the spiral currents were equal, and in the same direction throughout.

It now occurred to me to try the effect of using a single pole of the magnet; this was done whilst a constant current was passing through the wire, commencing at the extremity, where the positive joined, drawing the north pole through the length of the wire, from positive towards the negative; the effect was most remarkable, as the steel wire now gave out as loud tones as a piece of iron, and the degree on the coil showed 200°. The constant and intermittent currents now showed for either polarity a remarkably strong right-handed twist; the positive 200 right, and the negative 150 right-handed spirals; the molecular strain on its wire from the reaction of the electric current upon the molecular magnetism was so great, that no perfect zero could be obtained at any point, a fact already observed when a wire was under an intense strain, producing tertiary currents that superposed themselves upon the secondary. In order to compare these spiral currents with those obtained from a known helix, I found that taking a copper wire of similar diameter (0.5 millim.), and winding it closely upon the steel wire ten turns to each centimeter, having a total of 200 turns, with an exterior diameter of 1.5 millims., withdrawing the steel wire, leaving this closely wound helix free, that it gave some 190°, instead of the 200 of the steel wire alone; thus the spiral currents fully equaled a closely wound copper wire helix of 200 turns in a similar length.

If it were possible to twist a magnetized wire several turns to the right, and that its line of magnetism would coincide with that of the twist, then on passing a positive or negative current, there would be an apparent augmented or diminished spirality of the current, but both would have a right-handed twist. The result would be identical with the phenomenon described, although the cause is different.

The explanation of this phenomenon can probably be found in the fact that the constant spirality now observed is that of the electric current under which it was magnetized, for whilst magnetizing it we had a powerful source of magnetism constantly reacting upon the electric current, and the constant spirality now observed is the result or remains of a violent molecular reaction at the instant of magnetization, and the remaining evident path or spiral is that of the electric current. On testing this wire to its longitudinal magnetic force, I found that it was less than a wire simply magnetized in the usual way; thus the effects are internal, affecting the passage of the electric current, giving, however, no external indications (except apparent weakness) of the enormous disturbance which has taken place.

If, instead of drawing the north pole of the magnet as above, from positive towards negative, I draw it from negative to positive, all the effects are repeated, except we have now, as we should expect, a left-handed spiral, but if I draw the magnet from the extremities of the wire to the center, then at this center I find an absolute zero of twist, but on each side a contrary twist, the wire then having a left and right-handed twist, the positive traveling towards the center in a right-handed twist gradually ceasing in zero; this is as we might expect, but if done under the influence of a constant current, no mat-

ter what pole of the battery enters afterwards the north pole of the magnet, it will have during its first half a right-handed, and in its second a left-handed spiral. It became important to know if a wire, which had been magnetized under the influence of a current, could be restored to something like its original condition. Electric currents had no effect. Heat, which would not destroy its temper, had no effect. Mechanical vibrations and torsions failed to disturb the molecular arrangement; but by magnetizing it strongly by a magnet, when no current was passing, at once brought the wire to its usual apparently rigid state, and the constant or intermittent currents now indicated only 18° of spiral currents against a previous 200°, and the sounds were as usual, from steel, excessively weak. I have since used this method with invariable success, when I wished to preserve or repeat the experiments upon the same wire. If these experiments are repeated on an iron wire, the effects are far greater in the first instance, so great that they were thrown out of the range of my measurements; it was only after a few seconds of successive reversals that the zero of the wire was brought within range, and although these rapidly decreased, exactly similar effects were observed as in the steel, and as with all moderate ranges, I could bring the iron at once to a complete zero by torsion, and as torsion alone would produce this complete zero, I believe we have here effects from iden tical causes to those related in the first chapter.

Having noticed in my previous papers the increased molecular activity caused by the approach of a powerful permanent magnet, and believing that the permanent spirality above mentioned was due to this alone, and not to an increased polarity, I magnetized strongly an iron wire giving, as usual, a reversed spiral for different currents of but 10°. I now heated the wire by a spirit-flame to a dull-red heat, whilst the current was passing through it, and on cooling I found a similar but stronger permanent torsion of 250°; both currents, as in the previous experiments, have a right-handed spiral. Thus, a current of electricity passing through a wire, nearly red hot, determines molecular arrangement, or path, which, on cooling, forces currents of either direction to follow the path which had been determined under the influence of heat.

3. MOLECULAR SOUNDS.

The passage of an intermittent current through iron or other wire gives rise to sounds of a very peculiar and characteristic nature. Page, in 1837, first noticed these sounds on the magnetization of wires in a coil. De la Rive published a chapter in his "Treatise on Electricity" (1853) on this subject, and he proved that not only were sounds produced by the magnetization of an iron wire in an inducing coil, but that sounds were equally obtained by the passage direct of the current through the wire. Gassiot, 1844, and Du Moncel, 1878–81, all have maintained the molecular character of these sounds. Reis made use of them in his, the first electric telephone invented, and these sounds have been, since the appearance of Bell's telephone, often brought forward as embodying a new form of telephone. These sounds, however, for a feeble source of electricity, are far too weak for any applied purposes, but they are most useful and interesting where we wish to observe the molecular action which takes place in a conducting wire. I have thus made use of these sounds as an independent method of research, and by their means verify any point left doubtful by other methods, some of which I have already described.

The apparatus was the same as in the last chapter, except that no telephone was used. The intermittent electric current was connected by means of switch key, either with the coil inducing longitudinal magnetism in the wire, or could be thrown instantly through the wire itself, thus rapid observations could be made of any difference of tone or force by these two methods; a reversing key also allowed when desired a constant current of either polarity to pass through the wire under observation.

Iron, of all metals that I have yet tried, gave by far the loudest tones, though by means of the microphone I have been able to hear them in all metals; but iron requires no microphone to make its sounds audible, for I demonstrated at the reading of my paper, March 31st,

that these sounds with two bichromate cells were clearly audible at a distance. A fine soft iron wire (No. 28) is best for loud sounds to be obtained by the direct passage of the current, but large wires (1 millim.) are required for equally loud tones from the inducing coil. By choosing any suitable wire between these sizes we can obtain equal sounds from the longitudinal magnetism or direct current. The wire requires to be well annealed; in fact, as in all preceding experiments, the sounds are fully doubled by heating the wire to nearly red heat. There are many interesting questions that these molecular sounds can aid in resolving, but as I wish to confine the experiments to the subject of the two preceding chapters, I will relate only a few which I believe bear on the subject.

On sending an intermittent electric current through a fine soft iron wire we hear a peculiar musical ring, the cadence of which is due to that of the rheotome, but whose musical note or pitch is independent both of the diameter of the wire and the note which would be given by a mechanical vibration of the wire itself. I have not yet found what relation the note bears to the diameter of the wire; in fact, I believe it has none, as the greatest variation in different sizes and different conditions has never exceeded one octave, all these tones being in our ordinary treble clef, or near 870 single vibrations per second, whilst the mechanical vibrations due to its length, diameter and strain vary many octaves.

I believe the pitch of the tone depends entirely upon molecular strain, and I found a remarkable difference between the molecular strain caused by longitudinal magnetism and the transversal or ring magnetism produced by the passage of a current, for if we pass the current through the coil, inducing magnetism in the wire, and then gradually increase the longitudinal mechanical strain by tightening the wire, the pitch of the note is raised some three or four tones (the note of the mechanical transversal vibrations being raised perhaps several octaves); but if we tighten the wire during the passage of an electric current through it, its pitch falls some two or three notes, and its highest notes are those obtained when the wire is quite loose. A similar but reverse action takes place as regards torsion; for if the wire is magnetized by the coil we obtain an almost complete zero of sound by simply moving the torsion index 45° on either side, and as this was the degree which gave silence in the previous experiments for the same wire, it was no doubt due to the same rotation of its polarized molecules. If we now pass a constant current through the wire whilst the intermittent one is upon the coil, we hear augmented sounds, not in pitch, but loudness, and if we give torsion of 45° to one side we have silence, or nearly so, whilst the other side it gives increased tones which become silence by reversing the battery. If, whilst the wire by torsion has been brought to zero, we decrease or increase the mechanical longitudinal strain, then at once the polarized molecules are rotated, giving loud sounds; and we further remark that when the wire is loosened, and we again tighten it, we gradually approach a zero, and on increasing the strain the sounds return; thus we can rotate the molecules by a compound strain of torsion and longitudinal strain.

If we wish to notice the influence of a constant current passing through the wire under the influence of the intermittent current in the coil, we find that if the wire is free from torsion that on passing the current the tones are diminished or increased according to the direction of the current; the tones then have an entirely distinctive character, for whilst preserving the same musical pitch as before, the tones are peculiar, metallic and clear, similar to when a glass is struck, whilst the tones due to longitudinal magnetism are dull and wanting in metallic timbre. If we now turn the index of torsion upon one side, we have a zero of sound with or without the current; but the opposite direction gives increased tones whilst current is passing through the wire, but zero when not. Here again a peculiarity of timbre can be noticed, as although we have loud tones due only to the action of the current through the wire, the timbre is no longer metallic, but similar to that previously given out by the influence of the coil; evidently, then, the metallic ring could only be due to the angular polarization of the molecules, and when these were rotated by torsion the tones were

equally changed in its action upon the wire.

I have already shown that a permanent magnet brought near the wire could rotate its polarization, and it equally can produce sound or silence in these molecular sounds (during the time that the wire is at its zero of torsion, and a constant current sent through the wire as in the last experiment) we find that either pole of the natural magnet has equal effect in slightly diminishing the sound by an equal but opposite rotation from the line of its maximum effects; but if the wire is brought nearly to zero by torsion, then on approaching one pole of the natural magnet we produce a complete silence, but the opposite pole at once rotates the molecules to its maximum loudness, and on taking away the magnet we have comparative silence as before.

Heating the wire to nearly red heat by a spirit lamp increases the tones of longitudinal magnetism induced by the coil some 25 per cent.; but it has a much more marked increase on the tones produced by the direct passage of the current where they have more than 100 per cent. increase, and if we pass the intermittent current through the coil and constant through the wire, we find no direct rotation of the molecules by heat. Although an apparent rotation takes place if we by the required torsion first place the wire at its zero, then on the application of heat faint sounds are heard, which become again almost silent on cooling; this is simply due to the diminution by heat of the effect of the elastic torsion.

Tempered steel gave exceedingly faint tones, requiring the use of the microphone; but on magnetizing with a constant current, inducing spiral magnetism, the sounds became audible, some 15° sonometer against 175° for iron; thus the molecular rigidity of steel as observed by previous methods were fully verified.

I have mentioned only a few of the numerous experiments I have made by the three methods described, all of which, however, bear directly upon the molecular arrangement of electric conducting bodies. I have selected a few bearing directly upon the subject I have chosen for this paper.

I have, I believe, demonstrated by actual experiments which are easy to repeat, that:

1. An electric current polarizes its conductor, and that its molecular magnetism can be reconverted into an electric current by simple torsion of its wire.

2. That it is by the rotation of its molecular polarity alone that an electric current is generated by torsion.

3. That the path of an electric current through an iron or steel wire is that of a spiral.

4. That the direction of this spiral depends on the polarity of the current, or that of its magnetism.

5. That a natural magnet can be produced, having its molecular arrangement of a spiral form, and consequently reversed electric currents would both have a similar spiral in passing through it.

6. That we can rotate the polarized molecules by torsion or a compound strain of longitudinal and transversal.

7. That the rotation or movements of the molecules give out clear, audible sounds.

8. That these sounds can be increased or decreased to zero by means that alone have produced rotation.

9. That by three independent methods the same effects are produced, and that they are not due to a simple change or weakening of polarity, as when rotation has been incomplete a mere mechanical vibration has at once restored the maximum effect.

10. That heat, magnetism, constant electric currents, mechanical strains and vibrations, have all some effect on the result.

In presenting these results to the Royal Society, I desire simply to draw attention to the effects that molecular action can produce in its relations to electricity and magnetism, and it seems to me that a knowledge of the molecular actions taking place is a necessary step previous to knowing what magnetism is in itself.

A NEW locomotive with a "patent steam reverse" is being finished at Reading, Pa., for the Philadelphia and Reading Railroad. This, it is said, will be the first locomotive ever built in Reading with an attachment for reversing the engine by steam instead of by hand.

THE IMPURITIES IN WATER, AND THEIR INFLUENCE UPON ITS DOMESTIC UTILITY.

By GEORGE STILLINGFLEET JOHNSON, M. R. C. S., F. C. S.

From the "Journal of the Society of Arts."

THERE are some impurities found in the water of rivers, more especially in those rivers which, like that in the immediate neighborhood of this building, take their course through large towns, concerning which I shall have little to say this evening. I allude to organic impurities, the detritus of living beings, sewage, and the like, and my reason for keeping silence upon this great subject is the incompleteness of our knowledge regarding it. Our highest medical authorities seem to be at variance as to the nature and degree of the baneful influence exerted by those impurities which I have mentioned upon the human economy, with the exception of the so-called specific poisonous products of such diseases as typhoid and cholera; and our highest chemical authorities are very much at variance as to the best method of estimating or determining the amount of these organic pollutions in waters, as they also are in the various accounts they give of the processes by which nature removes them. It would ill become me, therefore, to do more than hint at the existence of this source of contamination of water, unless I stood prepared to bring forward some new facts or experiments throwing light upon the subject, which I am not in a position to do. I must, therefore, confine myself this evening to the discussion of some of the more important inorganic impurities contained in natural waters, and their influence upon the domestic utility of the important liquid which contains them.

The word "impurities" has occurred several times already in this paper. I have also spoken of "pollutions" and "contaminations," all of which expressions tend to convey the idea that the presence of substances so described, in the water we drink and employ for household purposes generally, must needs be injurious and prejudicial. Now, the tendency of this paper will rather be to show the great usefulness of many of these so-called "impurities" in natural waters; and the word is used here in its strictly chemical sense, to indicate anything which we find in and accompanying water which is not the chemical compound, H_2O.

Pure water, the compound containing two atoms of hydrogen combined with one atom of oxygen, is a pure chemical substance which is never found in nature. We explain this by the statement that water exerts a solvent action upon various gases and solids.

It is, then, by virtue of its solvent action that water becomes impregnated with the impurities of which I am to speak; and I will, therefore, ask you to follow me while I make a few preliminary remarks upon, and show you a few experiments illustrating, the nature of solution. The process of solution consists essentially in a change of physical state, without alteration of chemical constitution. Thus, when sugar or common salt is dissolved in water, we can obtain the solid sugar or chloride of sodium by simply evaporating the water; and these are instances of true solution; but, if metallic copper be dissolved in nitric acid, that is an instance of solution accompanied by chemical change; for, if we evaporate the blue liquid thus obtained, we have a deposition, not of metallic copper, but of nitrate of copper, the salt formed by the chemical action which takes place between that metal and nitric acid. Solution proper, then, consists in a change of physical state simply without change of chemical constitution. Now, we know of but three physical states in which matter can exist, the solid, the liquid and gaseous. The solvent or substance which brings other substances into solution is usually a liquid. The dissolved body may be either a solid or a gas.

Now, the physical state in which we find any substance depends to a great extent upon the nature and intensity of

the physical forces which happen to be acting upon it at the time. Besides the action of solvents, the two physical forces, heat and pressure, exert a powerful influence upon the physical state of matter. The essential difference between the three physical states of matter is one of the relative freedom of motion which exists between the molecules or ultimate particles of which the matter consists, the gaseous form of matter possessing the greatest, whilst the solid possesses the smallest degree of molecular mobility. Heat, on the one hand, increases this mobility of the molecular of matter, whilst pressure has the reverse effect.

Next, observe that the solvent (liquid water, *e. g.*) is in the intermediate condition, as regards molecular mobility, between the solid and the gas, whose physical state it must assimilate with its own before it can bring them into solution. It follows, then, that the liquid solvent must bind a gas in chains, as it were, must diminish the free mobility which exists among the particles of that most elastic form of matter, whilst it will have to increase the molecular mobility of the comparatively sluggish solid, in order to make them respectively assume its own physical state. Accordingly, we should expect to find that a liquid will have its solvent action upon solids increased by the application of heat, whilst its power of dissolving gases will be diminished by heat, but improved by pressure. And these laws are obeyed in almost all instances.

I will now show you one or two experiments, to illustrate these preliminary remarks upon solution. When I stir up these two white powders in separate beakers of hot distilled water, you observe that one of them (which is powdered sugar) becomes readily incorporated with the water, changes its physical state, assuming that of its solvent, is dissolved. That is an instance of a soluble substance. This other powder, however, refuses to do anything but remain partially suspended in the water, making the liquid look milky, whilst the greater part of it (for it is very heavy) sinks and remains at the bottom of the beaker. It is the salt called sulphate of baryta, and is one of the most insoluble bodies known.

To illustrate the effect of heat in assisting the solution of a soluble solid substance in a liquid, it will be sufficient to cool this hot saturated solution of iodide of lead, when we find that water, which was capable of retaining a large quantity of that salt in the liquid state whilst hot, becomes incapable of doing so as it cools, and the excess of salt separates out from the solution in the chrystalline form.

To demonstrate the action of heat in retarding the solution of a gas in a liquid, I will first pass up a little water into this tube (which contains dry ammonia gas confined over mercury). As soon as the water reaches the gas, you see that the latter disappears, being dissolved by the water. Now, if I pour a little hot water over the outside of the tube, we shall soon see the effect of heat in increasing the molecular mobility of the ammonia, for the restraining power of the water, at this high temperature, becomes insufficient to control the elasticity of its volatile companion, and the ammonia bursts its chain and resumes the gaseous condition. As the tube cools again, the solvent power of the water is again triumphant, and the gas disappears. Not only does the temperature of the liquid solvent exert an influence upon the quantity and quality of the substances which it is capable of dissolving, but the solvent action of a liquid is often considerably modified by the presence therein of substances which it has already dissolved.

We will consider this influence of dissolved matter in water upon its solvent action on other forms of matter somewhat fully, since it serves to explain the presence of some of the impurities found in waters; and it will be convenient to divide the subject into two heads, viz.:

1. The influence of dissolved gases upon the solubility of solids.

2. The influence of dissolved solids upon the solubility of other solids.

1. Excluding those cases in which a chemical action occurs, resulting in the production of some insoluble compound by the action of a dissolved gas upon one or the other of the elements present in a dissolved solid, the general tendency is for a dissolved gas to increase the solubility of solids in their common solvent. As an illustration of this, I will cover this solution of copper sulphate with a strong solution of ammonia gas in water.

You see now three layers in the containing vessel. Below the blue solution of copper sulphate, above the colorless solution of ammonia gas in water, and between the two, a light blue turbid layer, the turbidity of which is due to the presence there of suspended hydrated oxide of copper, a substance which is insoluble in pure water, and in most neutral and alkaline solutions, but which is soluble in a solution of ammonia gas in water, yielding a dark blue liquid, which you see is produced when I stir up the contents of the beaker. There are other instances which will occur to every chemist, of solid bodies quite insoluble in pure water, yielding to the solvent action of a solution of ammonia gas in water. It appears, then, that the dissolved gas confers a degree of molecular mobility upon the water which has dissolved it, or at least enables that water to produce the requisite freedom of motion amongst the molecules of an otherwise sluggish solid, which is necessary in order to compel it to assume the liquid state.

2. It is frequently observed, and especially amongst the halogen group of elements, that an insoluble salt is rendered soluble by the presence in their common solvent of a very soluble solid body. One of the most striking and beautiful examples of this is seen in the case of the red mercuric iodide, which is entirely insoluble in pure water, but is readily dissolved by water saturated with potassic iodide—a very soluble salt. It is essential that the potassic iodide be present in a somewhat concentrated solution, for, as you see in this beaker, when a solution of mercuric iodide in one of potassic iodide is mixed with a large bulk of pure water, the red mercuric iodide separates out. If there be any chemical action between the two iodides in this case, it is of the very feeblest kind. Indeed, some experiments of my colleague, Mr. J. M. Thomson, have tended to show that, if the double salts formed by dissolving insoluble halogen compounds in soluble ones be compounds at all, they are molecular, not atomic combinations. It is, at all events, interesting to remark, that when a dissolved solid assists the solution of another solid body, it is the more soluble substance—that which is endowed with freest molecular mobility—which serves to bring about the liquefaction of the more sluggish solid; and there are instances of this action which cannot be at all explained by chemical action, as in the case of the solubility of quick lime in a strong solution of sugar.

It sometimes happens that the action of a solvent is arrested by the formation of a protecting film of an insoluble substance upon the surface of an immersed solid. Thus, marble, which is a compact crystalline variety of carbonate of lime, is freely dissolved by a solution of hydrochloric acid gas in water, the only solid product of the accompanying chemical action being the salt known as calcic chloride. Now, calcic chloride is freely dissolved by water, and, as each particle of it is formed on the surface of the marble, it is dissolved off by the water, and fresh surfaces of marble are constantly exposed to the action of the hydrochloric acid. But, if we immerse marble in water containing both hydrochloric acid and sulphuric acid in solution, its surface speedily becomes covered with an insoluble film of calcic sulphate, and the action ceases. Marble is still there in abundance; hydrochloric acid is also present in quantity, adequate and sufficient for its solution; but, by reason of the intervening insoluble film of calcic sulphate, they are prevented from acting upon one another. "The chemical force can only act at infinitesimally small distances." Another instance of the protecting action of an insoluble film upon the surface of an otherwise soluble solid is seen in the case of the black ferrous sulphide. When this substance is acted upon by sulphuric acid, the salt known as ferrous sulphate is produced. Now, green vitriol, or ferrous sulphate does not dissolve in cold, strong sulphuric acid, but it dissolves readily in hot dilute sulphuric acid. When, therefore, I pour cold oil of vitriol over this ferrous sulphide, there is little or no action, a film of ferrous sulphate forming on the surface of the sulphide, and protecting the sulphide beneath from the action of the acid; but when I pour water into the containing vessel, a brisk action is at once set up, heat being developed by the admixture of the water with the acid, cold strong sulphuric acid being converted into hot dilute sulphuric acid,

which dissolves off the ferrous sulphate as fast as it is formed.

I will now pass on to a consideration of some of the impurities contained in natural waters—in water as it is supplied to us for use in every-day life—explaining, where that is possible, the sources and method of contamination, and, further, discussing the chief precautions necessary for the removal of such impurities as are prejudicial to the domestic utility of this valuable agent. First, then, we will consider the gas found in solution in natural waters, with some trifling exceptions, viz., some of the rarer mineral waters, the gases dissolved in water are those which are present in our atmosphere—oxigen, nitrogen, carbonic acid, and ammonia. The oxygen and nitrogen gases, the elementary constituents of the atmosphere, are present in it in invariable quantities, and are far less soluble than the other two.

The carbonic acid and ammonia, or compound gases, are chiefly products of animal life, and are constantly being removed by plants and vegetable organisms, but they are also more soluble in water than the first two. The carbonic acid is present in larger proportion than the ammonia, whilst it is also far less soluble than the latter gas. Indeed, after a long continued fall of rain, the presence of ammonia in the air of a place is hardly recognizable.

Spring waters are very apt to contain much larger quantities of CO_2 than rain water or river water. Meandering, as they frequently do, through subterranean passages, they are exposed in their course to influences peculiarly favorable to their conversion into strong solutions of this gas. The earth being the common receptable for dead organic matter, and her cavities being in many cases never penetrated by the sun's rays, or ventilated in any way, accumulations of carbonic acid are to be expected in these regions. The water, then, which is often very cold (it may be produced by melted snows), is churned up at frequent intervals along its course with these terrestrial gases, and becomes, in consequence, highly charged with them.

We are able to demonstrate the presence of dissolved gases in water, by simply boiling it in an apparatus such as this which I now show you, and collecting the permanent gas which escapes, as is being done here. The presence of these dissolved gases in water appears to be in every way beneficial. If we consider water as a beverage, the sparkling and refreshing effect of spring water is largely due to the dissolved gas, especially to the carbonic acid gas which it contains. Again, boiled or distilled water, from which the gases have been expelled by heat, is mawkish and insipid, but may be again rendered palatable by aerating it with charcoal. But more than this, absolutely gas-free water (which, however, can only be obtained by boiling water in vacuo), boils at a temperature considerably above 100°C., and with violent explosion.

Again, it is probable that the oxygen dissolved in water oxydizes, and removes some of the more readily putrescible organic matters contained therein; and it certainly is of the utmost importance to the life of fish. The dissolved gases in water also exert an important influence upon its solvent action for solids, as we shall now find. The solid substances dissolved in waters are generally chlorides, sulphates, and carbonates of the alkalies, and of the alkaline earth metals.

Those waters which contain the alkaline earths in solution, are divided into (1) calcareous and (2) magnesian waters, the former containing sulphate or carbonate of lime in solution, the latter sulphate or carbonate of magesia. Such waters are said to be hard. Now, it is in the case of the carbonated calcareous and magnesian waters that we observe most distinctly the influence which a dissolved gas may exert in modifying the solubility of a solid in their common solvent. For the carbonates of lime and magnesia are insoluble in pure water, or nearly so; but considerable quantities of these salts may be brought into solution by water charged with carbonic acid gas. For instance, if I bubble carbonic acid gas through this clear lime water, we first observe a milkiness, due to formation of the insoluble carbonate of lime; and on continuing to pass the gas, we finally obtain a clear solution. The dissolved gas enables the water to overcome the molecular sluggishness of the calcic carbonate, and to reduce it to the liquid condition; just as the dissolved ammonia gas in our previous experiment

enabled the water to hold in solution the hydrated cupric oxide. Now, if I boil this clear solution of bicarbonate of lime, the excess of gas is expelled by the heat (just as the ammonia gas was expelled from its dissolving water when the temperature of the tube containing the solution was raised), and the water, no longer aided by the mobile carbonic acid gas, loses its power of keeping the calcic carbonate in the liquid state; accordingly that salt is reprecipitated.

Bearing the facts in mind, we shall be able to explain some of the phenomena of nature in connection with this subject of calcareous waters. We have seen that spring waters are frequently highly charged with carbonic acid gas; now carbonate of lime, in the shape of chalk deposits and limestones of various kinds, is a very constant ingredient of the soil in many parts of the earth's surface. It must, therefore, be a matter of very frequent occurrence for water, already highly charged with carbonic acid gas, to come in contact with carbonate of lime in the course of its subterranean wanderings; hence the frequent contamination of natural waters with dissolved carbonate of lime. But there is another interesting and very beautiful phenomenon which we are enabled to explain by the light of the above facts. I mean the formation of stalactites and formations such as are figured in the diagram on the wall. Suppose a water holding in solution much carbonate of lime and carbonic acid gas to trickle slowly through the roof of a cave. From each drop of water, as soon as it finds itself exposed to the common air, some of its dissolved carbonic acid gas will begin to evaporate, and for each molecule of gas which thus leaves the water, a molecule of calcic carbonate will be deposited in the solid form. Let a few of these solid particles adhere to the roof of the cavern, and from the nucleus thus formed, the production of vast conical masses, such as are here portayed with their beautiful tapering apices pointing towards the earth, is only a matter of time. The nature and quantity of the dissolved salts in spring water will, of course, vary with the composition of the soil through which it has passed. Many mineral waters are of great medicinal value.

We will next consider the influence of dissolved lime salts upon the domestic utility of water. Is "hardness" in water prejudicial? If we consider the water as a beverage, the answer would be, No, The worst that hard waters have been accused of is that they produce a tendency to calculous formations in those who drink them. But I think the water drinker may answer to that charge, "Not proven." And, on the other hand, we cannot but remember that the metals calcium and magnesium, in combination with phosphoric and carbonic acids, play the important part of conferring the requisite degree of hardness and stability to our frame—are, in fact, the earthy constituents of the skeleton. But there is another purpose for which water is employed, viz., for washing, and which is hardly less important than that we have just considered. For this purpose hard water is certainly disadvantageous.

Soap contains fatty acids, which form insoluble compounds with the lime and magnesia in hard waters, and no lather will be produced till all the lime and magnesia dissolved in the water have been precipitated in this way. And this occasions a waste of soap.

Now, what is called the temporary hardness in water may be removed by boiling it. The expulsion of the dissolved carbonic acid gas by that means, leads to the removal of the calcic carbonate from solution in the water, and the hardness due to that cause is then removed. But the water may contain sulphate of lime in solution, which will not be removed by boiling the water. On the contrary, unless the water had been previously saturated with the salt, the evolution of steam in boiling would rather tend to concentrate its solution, and thus the permanent hardness due to this cause would remain. Moreover, there is a further objection to boiling water (except in small quantities) for the purpose of removing its hardness, since, besides the consumption of fuel which is necessarily incurred, the deposited calcic carbonate tends to form boiler incrustations, often of considerable thickness, upon the walls of the vessel employed for the purpose, and if they do not lead, as they have too often done, to dangerous accidents by their suddenly becoming detached, and producing explosive bursts of steam, by allowing the water to come

in contact with the strongly heated metal wall of the vessel, yet must invariably cause great waste of fuel, owing to their inferiority as conductors of heat. Therefore, the process of Mr. Clark, which is conducted without any application of heat at all, was a great boon to mankind, especially as it has the additional advantage of clarifying a water as effectually as any filter.

The problem before us is essentially this. How may dissolved calcic (and magnesic) carbonate be best removed from solution in water? *i. e.*, how may these salts be converted into suspended and insoluble matter with the smallest possible expenditure of time and money? We have seen that the method of boiling the water, though effectual, is objectionable on the score of expense, liability to accidents, &c. Now, in Mr. Clark's process, which I have said is preferable, the suspended insoluble calcic carbonate produced has to be removed by subsidence. There are two methods by which suspended matter is removed from water in nature, subsidence and filtration, and these processes are also adopted by man for the same purpose. Now, it is claimed for the method of purification by filtration that organic matters are oxidized by the substances employed, *e. g.*, charcoal, which has the property of retaining oxygen gas in its pores. But the process of Mr. Clark also undoubtedly removes dissolved organic matters from waters, the lime which is added acting as a mordant, and producing their precipitation. Mr. Clark's process is as follows: By adding quick lime or hydrated (slaked) lime to a carbonated calcareous water, the carbonic acid gas which is holding the carbonate of lime in solution, is first removed by combination with the added lime, and the carbonate of lime thus produced falls, together with that previously in solution, as a solid insoluble precipitate. The turbid water is left to clear by subsidence, and is afterwards drawn off freed from temporary hardness.

I have hitherto been speaking of what may be called unavoidable impurities in water—impurities, viz., which are introduced by natural processes which are beyond the control of man; but before concluding, I must allude, however briefly, to a very important accidental source of contamination of water, which is sometimes introduced by man himself, I mean the contamination of water with lead. And here we shall find that the influence of dissolved matters in any water is extremely important in modifying its solvent action upon this metal. Lead, from the ease with which it is worked, and the resistance which it offers to atmospheric action, changes of temperature, &c., has been found to be a very convenient metal wherewith to construct pipes for conveyance of water, and cisterns for its storage. But lead is dissolved in appreciable quantities by some natural waters, and the long-continued ingestion of that metal, even in very minute quantities, produces serious symptoms of disease in the human subject, so much so that the metal has given its name to at least two specific affections, lead colic and lead palsy. It becomes, then, a matter of the utmost importance to be able to state, from a knowledge of the ingredients of any given water, whether or not it will be safe for persons to drink that water after it has been stored in leaden cisterns—whether or not that particular water is likely to exert any solvent action upon the metal. This we are able, in many cases, to do. For it has been found that pure water, free from both dissolved solids and gases, has no solvent action upon lead. But water containing dissolved oxygen becomes impregnated with lead, oxide of lead being, to a certain extent, soluble in water.

1. Practical Deduction—Rain water, stored in lead, must not be used for drinking purposes. Again, when waters containing carbonates, and especially sulphates, in solution are stored in leaden cisterns, the metal becomes coated with an insoluble protecting film of carbonate and sulphate of lead, further action being thereby prevented, and the water does not become saturnine.

2. Practical Deduction—Carbonated and sulphated calcareous waters may usually be stored in lead with impunity. The film which forms on the surface of the metal should by no means be removed.

3. Waters containing nitrates and chlorides in abundance cannot safely be stored in leaden cisterns, since the nitrate and chloride of lead are soluble salts. The practical deduction from this is obvious.

In concluding, I hope I have convinced most of my hearers that though we do not drink pure water, it would be very much worse for us if we did, and that, whilst we may sometimes be inclined to ask, "Why is such a substance here?" we generally find at last that it serves some important purposes which had escaped our ken—in fact, that we are finally led to wonder at the Wisdom which works through intricate and complicated labyrinths to a perfect and simple end, and are forced to admire the ultimate tendency and result of even such seeming anomalies as the "impurities in water."

THE USE OF MARBLE AND SIMILAR MATERIALS IN ENGLISH ARCHITECTURE.*

From "The Building News."

The subject of my paper is the use of marble and similar materials in English architecture. Among these materials I include all those stones which have a slight degree of translucency and will take a polish, and which are valued in architecture either for their beauty and variety of color—as, for instance, the veined and breccia marbles, porphyries, serpentines, and some kinds of alabaster—or else for their delicacy and purity, and the closeness and fineness of their texture, as in the case of statuary, Parian, and white alabaster. I have chosen this subject because the opportunity we Englishmen have now for using materials of this kind in our work is something novel in English architecture, and because, unless we recognize this novelty, and try to ascertain the proper principles by which to employ this new material, we are not likely to employ it well. It is true that it was not an unknown material in Mediæval England. This could hardly have been so, for in Devonshire it is a common building stone, and every shower of rain makes the smooth flagstones of Plymouth look like slabs of polished marble. It would be more correct to say, that, though not unknown, it was almost ignored. And it was ignored not so much from choice as from necessity. M. Viollet-le-Duc observes that in France, where the use of marble during the Middle Ages was as rare as with us, if not rarer, romancists were fond of writing of marble palaces, and so forth, and that the chroniclers of the Abbey of St. Denis boast of the marble columns with which Abbe Suger surrounded the choir, which, however, turn out, after all, to be only of stone. It is most probable that the expense and delay of working marble was the real reason why it was neglected in a style which was the child of economy, and in an age when labor was costly and laborers few. However this might have been, in the earlier styles of our native architecture it is the fact that, with the exception of the simple grey marbles of Purbeck and others like them, which were employed chiefly in shafts, the use of marble was neglected. In the later periods of Gothic art, alabaster, which is soft and easily worked, came into use for monuments and reredoses, and similar small works; and, still later, marble was used a good deal in our English Renaissance, though even then chiefly in small work, such as monuments and chimney pieces. The marbles then employed seem to be chiefly foreign, and to have been regarded as a rarity and used with economy. It has been reserved to our own age, when steam transport by land and sea has relieved us of the necessity of using only local materials in our works, and when provincialisms in architecture seem destined to give way before a more general interchange of materials and modes of workmanship, to have marble brought to our doors so easily, and in such quantity and variety, that it fairly takes its place as one of our building materials.

Now, to those who take what I believe to be the right view of our position in the history of art—who recognize the fact that we live in an age which is necessarily eclectic; building up for ourselves

* A Paper read before the Architectural Association by T. Graham Jackson, M. A.

an art out of the teaching, not of one only, but of many bygone styles, all more or less unsuitable—and who look forward to the development, in due course, of a really modern art which will represent us better—the acquisition of a new material will be welcome as opening the way for a fresh departure in our practice. For every special need of our life which is met and satisfied, and every new material which is adopted and put to its proper use, must involve some modification of the adopted styles we work in; and each such modification, if made in accordance with true artistic principles, or, what is much the same thing, common sense, must result in genuine progress. There is no need to urge the use of marble on modern architects. Ever since it has been easy to have it, there has been no lack of demand for it, and this is not wonderful. The first impulse seems to have been given by the books of Mr. Ruskin and Mr. Street. Delighted to find himself allowed, and even encouraged, to indulge himself in the study of a branch of Gothic architecture which had been forbidden by the sterner masters of Gothic purism, the student rushed to Italy and found there wonders of color and exquisite finish of workmanship for which his Northern training had in no way prepared him. Accustomed to regard a 4-in. marble shaft as an extravagance, and an alabaster pulpit or font as a matter for glorification, he suddenly found himself in a country where marble was used as a common building stone. The sensation with which, for the first time, one stands in an Italian church, such, for instance, as that of St. Lorenzo at Genoa, where arches and walls of banded black and white marble are carried on closely serried ranks of massive columns of deep purplish red, is one of astonishment mixed with incredulity. And as the student travels further and becomes accustomed to see marble used universally for purposes where, in his less-favored land, common stone would have been employed, his satisfaction grows in proportion as his wonder lessens. To many students after an Italian tour—at all events, a *first* Italian tour—the return to sober stone is like disenchantment, and it is no wonder that they seize every opportunity of using the material which has charmed them under an Italian sky. With this desire I have no wish to quarrel. We may safely welcome the new material and use it freely, provided we learn to use it properly and do not forget that it *is* a new material in English architecture, and that as a simple consequence its adoption must bring with it new methods and principles of design.

I propose to-night to touch successively on a few of the principles of design by which the effects which we admire in the marble work of the past have been produced; and to show how, by the neglect of these rules, we moderns have constantly failed to succeed in making the most of our new material; and how we have sometimes even spoiled what might otherwise have been tolerably good work, by the improper use of a material which would have improved it had it been judiciously employed.

The first thing to be insisted on is that marble is not, as some seem to think, merely a more beautiful kind of stone, which may be used just where and how we might use stone, but with a correspondingly more beautiful effect. Marble has other differences from ordinary stone than its superior beauty. In the first place, it is generally much harder, and to work it involves infinitely more labor; in the next place, it has a slightly transparent quality, which requires special kinds of design to make one's work upon it tell distinctly; and again, in the case of colored marbles, we have a totally new field of design opened to us differing from any that mere stone masonry offers. All these points involve new considerations to one who has been trained exclusively in the Northern schools of Gothic architecture, which are distinctly stone styles. Their deeply-recessed orders, undercut mouldings, and elaborate traceries, the highly-relieved character of their decorative sculpture, especially the naturalism of some of it, are all derived from the use of stone, and are appropriate only to a soft, easily-worked material. There is nothing here to teach us how to make the best use of our new material. It is only from those who have been used to work it that we shall learn how to deal with it, and it is, therefore, to Italy that the student must look for instruction. In that country marble has always been a possible and

the favorite material, and the true principles of working it have never been entirely forgotten. From the time when the Gothic or Lombard builder first began to carve rude marble capitals of his own by the side of those he pilfered from the ruins of antiquity, down to the time of the Renaissance, when perfect knowledge and unrivaled skill in dealing with marble seemed to make that stubborn material as plastic as clay in the artist's hands, we find it employed sensibly and with proper economy—by which I mean that no labor was unprofitably spent on it, and that the workman succeeded in producing the desired effect with the least possible fatigue. The principles of work according to which this result has been obtained are simply those by which every good school is guided; they are the same which, working under different conditions, produced the totally different styles of France and England. Decorative sculpture north of the Alps owes its character to the stone in which it was cut, and that south of the Alps to the marble in which it was cut, because in both styles the workman recognized and respected the qualities of the material he was at work upon, and as he worked in the direction which these qualities suggested to him, his work fell naturally into such a shape as best brought out the beauty of his material with least labor to himself. For instance, the first consideration of a sculptor or mason in marble would naturally be the hardness of it, and the time his work would occupy him. Every deep line in the foliage of his capitals would have, first of all, to be drilled out with a succession of holes, which would afterwards be joined together by the chisel into a regular sinking. This laborious effort is of itself enough to confine the carver to a stiff and regular mode of design. Of what use would it be to him to think of imitating the wild freedom of the stone-carved capitals of the North, which have all the rapidity and boldness of sketch, where we see that a few vigorous blows have sufficed to give a depth of shadow that it would take a week to produce in marble, and where there is an easy play and variety of line that can only be had where the chisel travels easily and freely, and finishes at once? To imitate this free work in a stubborn material is an artistic mistake of the first order, of which examples are not wanting even in old work. Most of you may be acquainted with the spiral colonnettes in the north and south portals of Chartres Cathedral, which support, if my memory is correct, the statues lining the jambs. The hollows of these twisted shafts are stopped by exquisite little bunches of leaves carved with a freedom and grace that have seldom been surpassed. They were evidently struck off in a happy vein, and quickly finished while the mood lasted. Those of you who have seen the front of Orvieto Cathedral may remember the spiral shafts of the doorways with their lovely mosaic inlay. The architect has there tried a similar plan of laying natural leaves at intervals in the hollows of his shafts, but with a far less satisfactory result. The freedom of the French example is entirely wanting. The leaves look labored and heavy and leathery, for they took so long to work that all the zest of the original idea evaporated before they were one quarter finished. To illustrate the difference between the two modes of work in the two materials, I may turn to the difference between an etching and an engraving. The etcher's needle travels sweetly and smoothly over the varnished plate. He can sketch as he goes on, with the utmost freedom, and finish his work a hundred different ways, whereas the engraver has laboriously to plough out line after line, to lay them parallel to each other, and to work evenly, regularly, and methodically. Just so the marble carver. He must lay his lines firmly and positively in their right places, and work them out slowly and painfully. There is no rapid sketching for him; and therefore he cannot hope to succeed in those designs which answer best with rapid handling. In this I believe we find one key to the greater conventionalism of the decorative carving of Italian art, and perhaps even to that of Classic art itself. The stiffness of material led to stiffness of design, except in cases where labor could not be misapplied nor the sculptor's interest exhausted, as in the human figure. It would not be wearisome to carve a block of marble into a Corinthian capital, or one of those conventional capitals of the Early Renaissance, of which there is such an in-

finite variety, because its very regularity and symmetrical perfection precludes the idea of its being done otherwise than with care and pains, and in a considerable time. You are in no hurry to embody your idea before it escapes you, for you have laid down your outlines correctly from the first, and have only to fill them up. It is natural that work such as this, which takes a long time and considerable labor, should bear the stamp of laboriousness, careful arrangement and perfect execution, rather than that of facility and freedom; and when the mechanical labor is so great as it is in the case of marble, it is natural that a great part of the less important ornament into which it is worked should be mechanical too. Hence, no doubt, the frequency in marble of such conventional ornaments as the egg and dart, and similar enrichments of Classic mouldings, which occur in Italian work even during the Middle Ages. The freer design of Northern carving is enjoyable only so long as it can be cut freely, and to carve it in marble would be tediousness itself in comparison with carving the more conventional ornaments I have named. There is, however, a free outlet for naturalism in marble, of which neither the ancient nor the modern Italians have failed to take advantage. It lies in the opportunity of working in low relief. In common stone considerable relief is generally necessary, partly because most stones have a coarseness of grain which interferes with a very delicate finish, and partly because even if the stone is fine enough it is seldom so hard as to make a low relief durable enough to resist ordinary wear. But marble is so fine of texture that every touch tells, and so hard that the smallest amount of relief may be safely trusted to last. I think no decorative sculpture shows more astonishing ability and more perfect appreciation of the material employed than some of the slighter reliefs of Italian art. Efforts of perfect solidity and modeling are produced with the relief of a bare ¼-in., and some of the finer lines are actually not relieved at all, but marked by a slight sinking of the ground or incising of a line round them. Here the labor is in comparison slight, and in the arabesques which cover the surface of the monuments of the Renaissance there is the utmost ease and grace and freedom of line, and often the most charming naturalism. But, above all, the Italian found room for naturalism in the representation of the human figure. There the labor required by the material suits the nature of the subject. In figure carving there is no room for hasty suggestive work; all must be complete and well studied, and finish can hardly be carried too far. The superior artistic value of the work also seems to demand the most durable of materials, marble or bronze, and to repay any expense of time and labor. There is no fear of the artist tiring of work that he feels to be worth the trouble he bestows on it.

I turn now to another property of marble which distinguishes it from stone —its variety of color. This introduces us to entirely fresh considerations, even more foreign to our native styles of architecture than those of which we have been speaking. At the beginning of this paper I distinguished two uses to which marble can be put and for which it is valuable: 1. The beauty and variety of color of some kinds. 2. The compactness of texture and simplicity of color of others. Now, though from one point of view we are in either case dealing with the same class of materials, from another point of view we are dealing with materials of two totally distinct kinds. They are distinct as the beauty of form is distinct from the beauty of color. This is so simple a distinction that it seems at first sight hardly worth enunciating, and yet there is no more common error than to confuse it. Marble that is valuable for its color is valuable for that alone. Nothing else ought to be present to our minds when employing it. To carve it is, except in very rare instances, a monstrous violation of the first principles of art. If, then, a marble, besides being colored, has any *variety* of coloring, it is absurd even to work mouldings on it; and yet this is a thing that is done every day. The architect wants a marble base or capping, and fixes his affections on a lovely block of breccia mottled with yellows, reds and blacks, on a nearly white ground; or a scarcely less beautiful piece in which white and colored veins cross and recross one another or lie obliquely along the surface. On this he works the rolls and

hollows of his base, or the details of his cornice, and is disappointed to find the result is not equal to his anticipations. The lines of the coloring contradict and confuse those of the mouldings. One does not know which to look at, the lines drawn by nature or those cut by the chisel. The level lines which are necessary in such features for the proper architectural effect are upset by the irregular markings of the material, and the beauty of the markings themselves is destroyed by their being broken into by the mouldings, which distort them just as a drawing is distorted when rolled up. The mistake is simply this, that the radical difference between designing for form and designing for color has been ignored. The effect is almost as bad when strongly-marked marbles, such as many of the Devonshire varieties, are used for steps. The effect of a flight of steps (and there are few more dignified features in architecture) depends on the regularity and evenness of the lines, and the simple alternation of light and shade in tread and riser. All this is destroyed by veins that run obliquely, and so distort the horizontal lines, or by strongly-massed colors, which interrupt the simple breadth of light and shade. In all the instances which I have given, the architect, caught by the beauty of the material, has thought that it would be sure to beautify anything he might use it for, and, having used it for an improper purpose, he ends by destroying both the form of the object and the beauty of the material. No material is worse abused at the present day in this respect than alabaster. Alabaster costs less than marble in the block, and being much softer, costs less in labor, and, consequently, is much cheaper as a material, and is, therefore, readily accepted as a substitute. With a little trouble it may be had quite white; but, as a rule, it is more or less strongly veined with warm brown streaks; and, strangely enough, it is this colored alabaster that seems most popular, not only for plain work, but for sculpture. The absurdest effect is sometimes produced by its employment for groups of figures in a reredos, when the brown streaks run undistinguishingly across the face of an apostle or the drapery of a saint, blurring the features of one and confusing the flowing lines of the other in the most distracting manner.

The plain rule for all kinds of varied marbles is that they should never be used except for their color, and in such a way as to make the most of their color. They should be used in flat slabs, inlaid in panels, like pictures, or used for lining the surfaces of walls or large compartments of pavements, always in large enough pieces to allow the variety of the markings to tell; for, of course, if they are cut up into small pieces they will differ but little from self-colored marbles. One very sensible way of using them is by splitting and reversing them, so that the veins and markings form regular figures, like the patterns formed by a kaleidoscope. This was very commonly done in Byzantine work, and may be seen at St. Mitale, Ravenna, and St. Mark's, Venice, and also, in later times, as in the palaces Vendramin Calerghi and Corner Spinelli on the Grand Canal, designed by the family of the Lombardi at the end of the fifteenth century. Perhaps the only lawful occasion for the use of variegated marble for actual parts of the architecture is that afforded by columns. Their form is so simple and clearly defined that the irregularities of the marble cannot confuse it, and the cylindrical surface is as well adapted for displaying the beauty of the markings of the marble as a flat surface would be. But such marbles should only be used for *plain* columns. If the columns were fluted the beauty of the material would be seriously diminished. The rule to employ a colored marble, for the sake of its color only, will, to a certain extent, apply also to self-colored marbles, which have no variation of surface. The instances in which sculpture can be successfully applied to any but white marble are very rare, even if they are admitted to exist at all. I do not forget the Egyptian figures in black stone, porphyry, or granite, nor the red marble lions that carry the columns of the Lombard porches in North Italy. But it may be observed that these are all archaic works that stand very low in the scale of sculpture, being conventional and abstract to a degree which we are not likely to imitate, and, indeed, could not imitate without affectation. But the prohibition

against the use of colored marbles for our more developed sculpture need not prevent us from working mouldings on it, for if the color be but uniform the mouldings are as well seen in one color as in another, or even as in white. For bases, cornices, architraves, and steps, therefore, there can be no objection to the use of marbles, such, for instance as the Veronese red, which is employed with such good effect in the north of Italy, or the black, which gives such dignity to many interiors in the Low Countries.

Whilst we are on the subject of colored marbles, I would touch upon another rule, and a most important one; that is, to work with moderation, and a limited palette. The same rule applies, more or less, to all decorative arts. The breach of it has done more to degrade modern glass painting than anything else. Seven tints of glass, I believe, sufficed the glass painter of the thirteenth century, and with them, and no more, he produced those miracles of gorgeous color which we see at Chartres, Bourges, and Canterbury. A similarly restricted palette was enough for the wants of the artists of the fourteenth and fifteenth centuries. The result was a harmony of color and a simplicity of composition that were admirably suited to the purposes of architectural decoration. The modern glass painters have, I believe, the choice of about 400 different tints, and many of them seem to avail themselves of the liberty of choosing without restraint, not with the happiest results. The only modern glass that is worth looking at has been designed on totally opposite principles, with but few colors carefully selected and artistically composed. And in the same way in the arrangement of marbles there can be no greater mistake than to use many varieties in the same composition. There are very few designs for which three kinds of marble would not be enough, or in which there would fairly be room for more. The marble screens of the Low Countries afford a good instance of the satisfactory application of this principle. One of the finest of them—now removed, unfortunately, from its original site in the Cathedral of Bois-le-Duc, and preserved, fortunately for us, in our museum at South Kensington—is an example of the kind of screen I refer to, of which there are scores in the great churches of the Netherlands and French Flanders, beginning with the old church of Notre Dame at Calais. In this magnificent structure there are but three marbles employed—black, white, and red—and they are used according to the principles I have endeavored to put before you. The white is used for sculpture, the black (which is of one simple tone) for the moulded architectural work, and the red, which is variegated, though of a very sober tone, for the columns.

There is another lesson to be learned from the practice of the best schools as to the usefulness of working with moderation and self-restraint in a material which offers so many temptations to over display as marble. The beauty of some of the variegated marbles is so great, and their colors so powerful, that they often exercise a seductive influence on those who have had little experience themselves in the use of marble, and have paid little attention to the practice of those who have had more. Caught by the splendor of these more gorgeous varieties, they neglect the soberer kinds, and they arrange their magnificent materials so as to produce the most powerful contrasts and the most gaudy combinations. No marble has been more popular in modern churches than a brilliant light green Irish marble, which is in itself of an almost hopelessly impossible color to work with, but is often made still more offensive by being placed next a bright red. Such gaudy combinations as these are enough to vulgarize any design, and are quite at variance with the teaching of all the best schools, which employed not only very few marbles at a time, as I have just explained, but generally preferred those of soberer colors, and avoided strong contrasts. Indeed, when one is new to the study of Italian art, and when the first rapture at the wealth of marble that surrounds one is fresh, one is disposed at first to wonder why strong contrasts were so seldom resorted to, and why, when so many splendid marbles were to be had, the quieter specimens were preferred. A structure of white marble will often be relieved only by panels of pale breccia, as at the Giant's Staircase at Venice, selected evidently for their light color

and amount of white ground. At the Scuola di San Rocco, one of the most lovely and refined works of the early Renaissance, the principal staircase is designed with still greater delicacy, the white marble being inlaid with slabs of a pale warm grey. In the presence of work such as this it is only the young or inexperienced architect who will wish for more color and decided contrasts. Those who have any real sense of art will soon learn to appreciate at their true value these quiet harmonies and subtle combinations of tints. It is, however, only on a large scale that the Italian masters dreaded and avoided strong contrasts. In their smaller inlays and mosaic patterns they sought brilliancy and distinctness, and used the strongest contrasts, not only of color, but of light and dark. Strangely enough, it is in small work such as this that modern men seem to choose marbles that will not contrast, and arrange them so as to produce no decided effect. That disagreeable Irish green, of which I have spoken, is often used next to a piece of pale red, without that intervening white or black which would have prevented their running together, and the result is a confused piece of color that has none of the brilliancy that is essential to design in mosaic. The true masters of mosaic worked in a very different way. They chose strong positive colors, the dark red of porphyry, and the deep green of serpentine, if they could get them, and almost always separated them by white, or pale yellow. A beautiful effect is produced in the inclosure of the stalls at the Baptistry of Pisa by simply inlaying a dark green marble in the white marble of the inclosing wall, so as to form a pattern in dark green and white, of which the white is the actual material of the ground left between the dark inlaid pieces.* This kind of sharp, trenchant design is the descendant of the old Lombard inlaid work, such as that in the Duomo and San Michele at Lucca, and San Miniato at Florence, and it was continued in a later form in the incised work filled in with black marble or cement, of which there are examples in the doorways of the Palazzo Communale at Cremona, the screens at San Petronie at Bologna, and some fountains now in the South Kensington Museum. Here, again, should be noticed the very limited number of colors with which these mosaics or inlays are worked. In the glorious pavements of opus Alexandrinum which cover the floors and add so much to the beauty of the Roman basilicas, and innumerable other churches in Italy, and whose beauty is not less remarkable than their variety of pattern and design, there are seldom more than three kinds of marble used—red porphyry, green porphyry or serpentine, and white; the white, however, generally cut from a marble which has veins of other colors in it, so that the piece which, according to the pattern ought to be white, is sometimes yellowish or pinkish or grey instead, and a certain amount of variety is thereby obtained, which prevents the hardness of a perfectly regular mosaic. The patterns thus produced are inlaid in a plain white marble field, and with these simple materials effects are produced which are not only harmonious, but constantly novel, each pavement having its own distinguishing character, by which it can be distinctly remembered.*

I turn now to another rule for the employment of marble, which may not be generally accepted, but which seems to me of no less consequence than the rest. Marble differs from stone, not only by its superior hardness and by its qualities of color, but by its semitransparency and the polish which it takes. The combination of these qualities, but especially the last, makes it difficult to use marble and stone together in the same design. Carving in stone must be free, that in marble severe: design in stone must be well relieved, and expression must be given to it by architectural features; design in colored marbles must be plain and flat, and characterized by smooth surfaces of color; and on these grounds alone the two materials can scarcely be used safely together. But the contrast between the opaque granulated texture of stone and

* Several examples of inlaid work similar to this, in dark green and white, or with the addition of a dull red as a third color, are illustrated in Waring's "Arts connected with Architecture in Central Italy." Plates 22 to 30.

* Several pavements of opus Alexandrinum are illustrated in Sir M. D. Wyatt's "Geometrical Mosaic of the Middle Ages" (*vide* plates 1 to 7). Restraint in number of colors may also be observed in the illustrations of decorative art in other materials in the same volume. See also Gruner's "Specimens of Ornamental Art."

the polished semitransparency of marble is so great, and the effect, when they are placed side by side, is so disagreeable, as to be intolerable to an educated eye. If the marble is not polished, the contrast is more endurable; but then, of course, the full beauty of the marble is not brought out, and it is no better than colored stone. It is less intolerable when marble is used for detached shafts, though even then it is a question whether simple stone would not have been better. The instance of the frequent use of Purbeck shafts in our English cathedrals is, I am aware, an authority that may be adduced against my rule; but I venture to think that even they are not always an improvement to the design; and it should be remembered that originally they did not stand as they now do,—stark and sharply-defined against white stone,—but were supported by colored decoration on wall and pier, which has now disappeared. But if the sober grey of Purbeck may be forgiven, what shall we say for the multitude of marble shafts of all colors and sizes that one sometimes sees in modern churches?—round the pulpit, perhaps, a dozen little colonettes, 2in. in diameter, and round the font half as many more, all of different kinds and colors, reminding one of nothing so much as the samples of polished drawing pencils in a colorman's window; and then, perhaps, on the piers or the jambs of the chancel arch, large shafts of Cornish serpentine that look nearly black and shine beautifully, all standing in hard cold relief against common stonework, and grating on the nerves by the disagreeable contrast between its dead surface and softened outlines and their glossy polish and hard positive forms? It is a safe rule that marble and stone should not be combined in the same design. If ever it is necessary to bring them near one another, let there be a strong definite line of separation, and let there be all marble on one side of it and all stone on the other; so shall we minimize the unpleasant effect which almost always arises from the combination of materials so different.

These, then, are a few of the lessons which are taught us by the nature of marble and the other materials which I have classed together with it, and by the practice of those who have every claim to be considered as our masters in the use of them. Let me briefly recapitulate them:

1. Decorative carving in marble, as, for instance, in cornices, capitals and friezes, where high relief and bold design are required, should be severe and conventional. Naturalism is forbidden by the stubbornness of the material except in the highest subjects, such as the human figure, which repays the necessary expense of labor, or else in very low reliefs when the labor of execution is reduced within moderate limits.

2. Sculpture should be in white marble, or, if in alabaster, only in such as is free from veins or stains of color.

3. Moulded architectural features, such as bases, bands, strings, cornices, architraves, abaci, should be either in white or in some uniform color without markings or veins.

4. Variegated marbles should be used only for panels or columns, or, in other words, on plain smooth surfaces, either flat or curved, so as to display the beauty of their markings to the utmost, without interfering with any of the structural lines of the architecture.

5. Colored marbles should be used in moderation, too great a variety being avoided, and those of the quieter and more harmonious tones preferred for general use.

6. Strong contrasts of color on a large scale are dangerous, and generally incline to vulgarity.

7. Strong contrasts on a small scale, as in mosaics and inlaid work, are necessary.

8. Stone and marble should be kept apart as much as possible.

In conclusion, it will be useful to consider how far, with these principles to work upon, marble is suitable to our English architecture. Most of the rules which I have just recapitulated, and which are derived from observation of the material itself, and the modes of using it which have been suggested and taught by its natural qualities, seem to point rather to Classic than to Gothic design. Severe conventional foliage, restraint imposed on freedom of representing natural forms, large plain wall-surfaces, and large columns, all seem to belong to Classic architecture rather than to our native Northern styles, which de-

light in naturalism, break up the wall surfaces with variety of architectural features, and compose their piers of small clustered shafts in preference to single columns, being in everything the very opposite of the mode of design which marble seems to require. And, indeed, I have no doubt, as I have already implied, that the qualities of marble have had as much to do with forming the character of Classic architecture, as those of stone with that of Gothic architecture. The very forms of Classic mouldings, with their square reveals and absence of undercutting, seem derived from the difficulty of working marble into hollows, and the necessity of right-angled sections, to throw distinct shadows in a partially translucent and strongly reflecting material. But it does not follow from this that, unless we give up Gothic architecture, we must give up the use of marble; nor even that we must give up our Northern Gothic and adopt that of Italy, which is in many respects a marble style. Our native Gothic is the result of our native climate, and our own natural character; and I for one believe firmly that no style will ever firmly root itself among us which is not at bottom based on the art which sprang up among us spontaneously, and which in country districts retained an obscure lingering existence all through the reign of pure Classicism and down to our own days. But our architecture may still remain true to Gothic principles in spite of our adoption of materials and conditions for using them that are new to the Gothic of the past. For instance, the art of the Early Renaissance among us is much more a Gothic than a Classic art. The mansions of Kirby, Hardwick and Burleigh, in England, and those of Chambord and Blois, in France, have nothing in common with Whitehall or Versailles. They are in principle Gothic buildings, in spite of the features and ornaments they have borrowed and adapted from Classical architecture. It was not until the Gothic element in the Jacobean style dropped out, and the temptation to indulge in the glories of pure Classic architecture triumphed, that we ceased to have a native style of our own; and we lost it then not because we carved acanthus leaves in our capitals, and made our shafts to taper, and copied the sections of Classic mouldings, but because we adopted the principles of Classic construction. Now we too, like our forefathers in the 16th century, have a choice of styles before us. Eclecticism is forced upon us. Unless we work like pedants we cannot help being influenced by other styles than Gothic, even if we make that the basis of our work. We must be bigots, indeed, if we refuse a noble and beautiful material because we find the rules for employing it to advantage are different from those that are proper to the stone we have been accustomed to. We should rather, as I said before, welcome it as an opportunity for a fresh departure, as a lever for lifting our modern work out of the groove of mere imitation of one style among the many from which we have to learn our lesson. On our architecture externally the adoption of marble can have little effect. Our climate forbids its use. Even in Italy the conglomorate marbles and breccias become disintegrated by exposure to the weather, and in England it would be folly to use them out of doors. White statuary is with us scarcely more durable, and though that harder kind of Carrara which for some mysterious reason we are in the habit of calling Sicilian is more durable, it soon loses in the external air all the appearance of marble, and looks little, if at all, better than any other white stone. Colored marbles, too, are of little use to us for external decoration, for their polish soon disappears, and with it their peculiar beauty, and they soon become less effective than many ordinary stones that are available for the same purpose, such as the red magnesian limestone of Mansfield, the grey Forest of Dean, the red sandstones of Bristol and other places, and the purple sandstones of St. David's, all of which have the advantage over marble of durability under exposure to an English climate. But in the interior of our buildings there is ample opportunity for the use of marble. It is, indeed, the only stone which is tolerable in positions where we must touch or brush against it. The passion of the earlier Gothic revival for showing honest building material within doors has fortunately run itself out; it was never warranted by the usage of the original style, and it was

found practically inconvenient. Our stone chimney pieces came off white on our clothes, grew smokey, and could only be cleaned by rubbing or scraping away the material; our stone fenders became dirty, and people wore them away by putting their feet upon them. And in more important architectural features it was found more comfortable to come into contact with wood and plaster than with bare stone. To marble, however, none of these objections apply. It is perfectly cleanly, and so hard that wear has little effect upon it within doors; and though in private dwellings its costliness must prevent any very large use of it, it need not exclude it altogether, while in public buildings nothing can be more appropriate or beautiful than marble for door cases, isolated piers or pillars, staircases, balustrades, pavements, and in more important buildings linings of wall surfaces, and even the structural members of the architecture itself. Of its suitability to church architecture there is no need for me to speak, for one seldom enters a modern church without finding marble used somewhere, though not always with good taste and judgment, nor in the right place. My subject has, perhaps, novelty to recommend it to your notice; for, largely as marble is now employed, there has, so far as I know, been but little attempt made to reduce the method of using it to any definite principles. We have rather dropped into the practice of employing it as we think best, without any general rules to guide us, and, consequently, we often employ it very absurdly and irrationally. It is time we began to consider the matter more scientifically, to apply to this new material those general principles of common sense and utility by which we admit all good art is governed, and thereby to arrive at some general rules by obeying which we may avoid mistakes for the future.

HYDRAULIC EXPERIMENTS AT ROORKEE.*

The report of the experiments of Capt. Cunningham, recently published, form an important addition to the literature of hydraulic engineering, continued through a period of nearly four years, under exceptionally favorable circumstances, and, prompted by a desire to contribute to our present knowledge of this department of practical engineering. These experiments possess a value not easily estimated.

The design of the work was conceived by the author while he was Assistant Principal in the Thomason C. E. College at Roorkee. It appeared to him that the neighborhood of the great Ganges Canal presented an unusually favorable opportunity for experiment, provided the proper superintendence could be secured. He accordingly proposed to Government a scheme for such work and volunteered the personal superintendence.

The Ganges Canal, the site of these experiments, is the largest canal in Northern India. It is 350 miles from its head at Hardwar to its outfall at Cawnpore. In its upper portion it is about 190 feet wide and 10 feet deep, discharging about 7,000 cubic feet of water per second at full supply; it decreases gradually in breadth and depth as it parts with its water for irrigation purposes. These irrigation channels, technically called Distributaries, of 20 feet in width, as well as the so-called Branches of 70 feet, are of themselves fair-sized canals.

The site selected for a large portion of the systematic work was that section of the canal extending across the valley of Solani River, a distance of $2\frac{3}{4}$ miles. The canal is here carried along the so-called Solani embankment; an earthwork having a width of 242 feet at the top and 317 feet at the base. For a length of $1\frac{7}{8}$ miles in one place and $\frac{1}{2}$ mile in another, the cross section of the embankment is quite uniform. Each side of the canal bed here consists of 12 masonry steps plastered with fine plaster, the lowest step having a rise of four feet, and each of the remaining eleven a rise of nine

* Roorkee Hydraulic Experiments, by Capt. Allan Cunningham, R. E. Honorary Fellow of King's College, London. Roorkee: Published at Thomason College Press.

inches, and all having a tread of fourteen inches. The width between the lowest steps is 150 feet.

Over the Solani River the canal is carried by a fine masonry aqueduct bridge of 15 arches of 50 feet span and a width of 192 feet. The aqueduct is here divided into equal waterways, each of 85 feet width and 932 feet long. The cross section of either aqueduct is a rectangle of 85 feet width. The surface exposed to the water is fine plaster laid over the brickwork.

Most of the experiments were made in the right-hand one of the twin aqueducts.

The primary objects of the work were: 1st. The discovery of a good method of discharge measurement. 2d. Testing the applicability of known mean-velocity formulas. 3d. Discovery of a good approximation to mean velocity for large canals.

The experiments were specifically directed towards the determination of the following data, viz.:

The velocity.
Unsteadiness of motion.
Surface slope and convexity.
Vertical velocity curves.
The proper function of rods.
Transverse velocity curves.
Mean velocity.
Discharge verification.
Silting.
Evaporation.

The results throughout have been compared with those estimated from the obtained data by the use of the several well-known formulas.

Of the four years work pursued under all the conditions afforded by the locality, and with all the various methods and instruments usually employed for the purpose, the author presents the following summary:

SUMMARY—INTRODUCTION.

Existing modes of discharge-measurement of large bodies of water are of doubtful accuracy.

New experiment is wanted on large bodies of water in motion under simple conditions, as in large regular canals.

A site to be very favorable for experiment should be situated in a straight uniform reach of great length, that is, with uniform banks, uniform bed, and uniform bed slope for a great distance above and below; and to be suitable at all for experiment must not be situated in a marked hollow in the bed slope.

VELOCITY MEASUREMENT.

Velocity in hydraulics usually means *forward* velocity, or the resolved part of the actual velocity taken parallel to the current axis.

Mean velocity measurements of all kinds are not sensibly affected by errors in estimating depth or breadth, but only by errors in the primary velocity measurements on which they depend.

In using floats, neither obliquity nor crookedness of path necessarily interferes with their proper use.

Floats measure the average of the forward velocities of the fluid particles successively displaced along the float path.

Floats can be used with advantage only at a favorable site, and not near irregular, nor very close to, any banks, however regular.

Floats have many advantages over *fixed* instruments, viz.: they interfere little with the natural motion of the water; they measure velocity directly; can be used in streams of any size; are little affected by silt or weeds; measure forward velocity; can be made and repaired by common workmen, and are cheap.

The ropes defining the run must be stretched at the lowest possible level.

A moderate deviation from the float course is admissible. This deviation being greatest in mid channel, decreasing slowly towards the banks, and rapidly close to the banks.

Strict accuracy in the position of the pendants is not essential.

To save time the dead-run must be reduced to a minimum near the banks, especially with surface floats.

Sub-surface floats require increased length of dead run as the depth of submergence increases.

For accurate timing the eye should be free to watch the visible phenomena, while the timing should be wholly done by the ear.

Similar observations at the beginning and end of the timing should be done by the same observer, so as to eliminate personal equation.

Precision in timing admits of short

runs which both save time and conduce to accuracy of course.

Long runs waste time. The run should be the shortest compatible with good timing.

With really good timing a 50-foot run is a good standard for general use; smaller ones must be used near the banks.

No float which is not a good float should be recorded. The sole criteria of a good float (the float's passage) being that it runs free and in fair course.

DETAILS.

The mean of highest and lowest free-water levels within a short interval, may be accepted as the average free-water level at the time.

This free-water level is slightly higher than the still-water level. For great accuracy, such as slope measurement, the free-water level should be taken. For ordinary work either the free or still-water level may be used; the same one to be used constantly at the same spot.

The average free-water level on either bank may be accepted as that of the site, except in case of high cross wind; then the mean of results on both banks may be taken.

The mean of the initial and final water level of an experiment may be accepted as the mean water level of an experiment.

In rough beds average cross sections should be determined from the average of several soundings along the course, and average depths determined from these should alone be used in computation.

The determination of average depths depends ultimately on the water level determination, so that the depths are liable to an error of a few hundredths of a foot, if the water level be taken from one bank only in a high cross wind, and this error would effect all computed results in which the depth entered as a factor.

Soundings should be taken whenever possible, with sounding rod from a boat floating freely down stream.

UNSTEADY MOTION.

The velocity at any one point of water is very variable, both in magnitude and direction, and the variation is very rapid; that is the motion is essentially *unsteady*.

Single velocity measurements are of very little practical use. Average velocities are the only results of much practical use.

Hydraulic experiments on large bodies of water must necessarily be both tedious and expensive, from the tediousness of determining average velocities.

Unsteady motion necessitates the use of a large stock of floats.

When velocity measurements at numerous points are required, the external conditions will probably change in the time required for obtaining so many average values. To meet this difficulty, the work should be done in *sets* of a few velocity measurements only, and should be done as rapidly as possible at each point in turn.

The details of each set will be under nearly the same external conditions, and the average results of many such sets, under nearly the same average external conditions.

The mean of about fifty velocity measurements, done in rapid succession, may be accepted as an average velocity measurements.

Only such sets as are under tolerably similar external conditions, should be combined into series. The combination should be so as to eliminate personal equation.

The average velocity curves for sites of regular contour in a long uniform straight reach are pretty regular curves, generally convex down stream. The departures from regularity in actual diagrams, are probably due to insufficient number of measurements, and to irregularity of contour of bed and banks at or near the site.

Discharge measurements or velocity measurements taken from single sets, are only fair average values.

The stream lines of water in motion interlace freely in all directions. There is an average steady motion. The unsteady motion is analogous to that of the wind.

The property of unsteady motion enormously increases the difficulty of forming a rational theory of fluid motion.

SURFACE SLOPE.

Surface slope measurement is an extremely delicate operation.

The slope length should be the shortest, yielding a surface fall greatly exceeding the ordinary oscillations of the free level.

The water level at the two slope points should be taken simultaneously. The points should be equidistant from the center section of the experimental site, in positions free from eddies and back waters, where the motion of the water is as quiet as possible, and with nearly equal surface velocities past them; the channel also should be symmetrical, both geometrically and physically, about the site throughout the slope length, and for some distance above and below.

Different slope lengths give different results, so that it is impossible, probably, to obtain true local surface-slope measurements. The same slope length should be used at any one site.

The surface slope of opposite banks are generally unequal. Local surface slopes should be deduced from simultaneous measurements on both banks.

The local surface slope does not change in any obviously regular manner with change of depth. It partakes of the changes of the surface falls of the upper and lower sub-reaches. It decreases rapidly with increase of obstruction at tail.

At times of high-water level, with no temporary obstruction at tail, the free surface sinks as follows. In nearly parallel lines in the upper sub-reach; in converging lines (with gradually flattening gradient) in the lower sub-reach; obstruction at the tail flattens the free surface gradient for a long distance back; with greatest effect in the region below the level of the crest of the obstruction, and with rapidly decreasing effect beyond this.

The surface gradient is chiefly determined by the control (chiefly obstruction) at the tail.

SURFACE CONVEXITY.

The measurement of convexity or concavity across the free surface is an exceedingly delicate operation.

The water surface in a long straight reach, with pretty straight banks, is, on the average, nearly level across.

SUB-SURFACE VELOCITY INSTRUMENTS.

Twin floats do not, in consequence of the unsteady motion, give a proper value of the velocity in the path of the sub-float.

The most serious fault of the double float is the rapid increase of connector resistance with increased depth of the sub-float. Other faults can be removed by suitably proportioning the parts of the instrument.

The efficiency of a given double float decreases with increase of depth of the sub-float, and there is a limit of depth beyond which it fails. To secure equal efficiency at all depths, the sub-float should be increased in size and net weight as the depth increases.

The sub-surface velocity measurement, made with the double float, is attributed to a depth always greater than the real depth of the sub-float, and is always more or less affected by the surface float and connector resistances.

VERTICAL VELOCITY CURVES.

Frequent re-adjustment of the connectors of double floats is inconvenient. Sub-surface velocity measurements, with double floats, can therefore only be done with convenience at fixed depths.

The properties of the average vertical velocity curves are as follows: The curves are generally convex down stream (except near an irregular bank); the maximum velocity is usually below the surface; the maximum velocity sinks in a rectangular channel from the center towards the banks, and is about mid-depth near the banks; the velocities near the bed are generally the least; the mid-depth velocities are generally greater than the mean; the curves are decidedly flat; the flatness decreases in a rectangular channel from the center towards the banks.

Vertical velocity curves obtained with the double float are distorted by instrumental defects as follows:

1*st*, *Max. Velocity at Surface.*—The partial errors are cumulative; the observation curve lies wholly without the true curve, and the error increases with the depth.

2nd, *Max. Velocity below Surface.*—From the surface to the maximum velocity line, the partial errors are cumulative;

from the maximum velocity line to a point where the velocity is equal to the surface velocity they are partly compensatory; below this line they are again cumulative. From the surface downwards the observation curve lies within the true curve, crossing it at a point somewhere in the second region above named; below this point it lies wholly without the true curve and the error increases with the depth.

The observation curves obtained with the double float are all too flat, especially near the bed, where the velocities are all exaggerated.

The mid-depth velocity on any vertical is subject to great and rapid variations from instant to instant, but to a less extent than the surface and bed velocities.

The bed velocity is subject to much irregularity.

Any marked change of any one velocity is accompanied, on the whole, by a *similar change* of all the velocities past the same vertical.

VERTICAL VELOCITY-CURVE FIGURE.

The investigation of the figure of the vertical velocity curve is a very delicate inquiry, as the data available (velocity measurements) are not good data for the purpose.

The method of trial and error is altogether unsatisfactory for the purpose, if the depth of maximum velocity line and parameter of the curve are to be discussed. The method of least squares is alone satisfactory in such case.

The average vertical velocity curve approximates in general closely to a parabola with a horizontal axis.

The data do not admit of the determination of the depth of maximum velocity line and parameter of the curve (which define the position and size of the curve) with any closeness.

The maximum velocity line is usually above the mid-depth on all verticals, and above one-third the depth on verticals at or near mid channel.

The tracing of the dependence of the maximum velocity depth and the parameter of the velocity parabola, on external condition, is very uncertain.

The Mississippi and Bazin experiments formulæ, for the parameter, are not based on good evidence.

DEPRESSION OF MAXIMUM VELOCITY.

The position of the line of maximum velocity on any vertical does not depend sensibly on the depth of the water, nor yet on the state of the wind. Wind must be long continued to sensibly affect the position of the maximum velocity line.

The air surface is a part of the wet border, causing a slight but sensible resistance to flow. The depression of the maximum velocity line is due largely to the air resistance.

DISCHARGE PAST A VERTICAL.

The arithmetic mean and trapezoidal rules for areas both err in defect on the whole.

The discharge past a vertical, found by use of a double float, is greater than the true discharge when the maximum velocity line is near the surface, and approaches equality with it as that line sinks to about one-third the full depth. As this line sinks further the discharge measurement falls short of the true discharge, and the discrepancy increases as that line sinks.

MEAN VELOCITY PAST A VERTICAL.

The arithmetic mean of velocities past a vertical is less than the mean velocity.

The mean velocity past a vertical varies less than most of the velocities of which it is the mean, but it is by no means constant.

The mean velocity measurement exceeds or falls short of the true mean velocity, according as it is less or greater than the surface velocity.

For finding mean velocity past a vertical by velocity measurements *at more than one point*, the formula, mean vel. $= \frac{1}{4}(V_0 + 3V_{\frac{2}{3}H})$ combines accuracy with the greatest practical convenience. (In this formula V_0 is the surface velocity, and $V_{\frac{2}{3}H}$ is the velocity at two-thirds the depth.)

The mean velocity is always below the mid depth.

The mean velocity past a vertical can only be approximated to by velocity measurements at a single point.

The average mid-depth velocity is generally greater than the mean of its vertical.

The ratio of mean to mid-depth velo-

city is not constant, but liable to about 16 per cent. variation.

The mean velocity past a central vertical increases and decreases on the whole, with increase and decrease of either depth or surface gradient.

The relation of the mean velocity to the external conditions is too complex to be worth endeavoring to trace out by mere experiment; that is, without some guide from a rational theory.

A much closer approximation to the central mean velocity may be obtained by direct velocity measurement of any one primary velocity past center vertical, than from any known formula in terms of surface gradient.

The best practical mode of mean velocity measurement in depths over 15 feet is to attempt only approximation by velocity measurements at $\frac{6}{8}$ depth, as a general rule, or $\frac{1}{10}$ depth close to vertical banks.

RODS.

The full length of a rod should only slightly exceed its immersed length. A long rod is quite unfit for use with small immersion.

Top hooks are a useless addition to a rod.

The rod-velocity of a rod nearly grazing the bed gives an approximation to the mean velocity past the vertical, generally closer than that given by the double float.

Rods move more steadily than any other sort of float.

The advantages of rods are, as compared with the double float, as follows:

They are free from the uncertainty attending the instability and unknown lift of the double float. They give an approximation to mean velocity past a vertical usually closer than that given by the double float. They give the result more rapidly. They are more easily handled and less delicate. They are simpler in construction, cheaper and more durable.

For measurement of mean velocity past a vertical, the rod should supersede all other instruments in cases favorable to their use.

The conditions favorable to the use of rods are: A reach of nearly uniform cross section and average bed scope throughout great length. The bed should be tolerably even length ways at and near the site. The depth should not exceed about 15 feet at and near the site.

The site should be prepared for the use of rods, as follows: The bed should be dressed to a tolerably uniform cross section and longitudinal slope for a length of, say, 250 feet. The banks should be dressed to a tolerably uniform slope for a length of, say, 250 feet, and, if likely to suffer erosion, should be revetted with masonry.

THEORY OF ROD MOTION.

The rod velocity is, within the limits of practice, always somewhat more deeply seated than the line of mean velocity past its immersed length, and is, therefore, within the limits of practice, always *somewhat less* than the mean velocity past its immersed length. For mere accuracy of measurement of mean velocity past a vertical, the immersed length of a rod should be *decidedly less* than the full depth on the vertical.

The proper immersed length of rod is from .950 to .927, or an average .94 of the full depth of the water.

TRANSVERSE VELOCITY CURVES.

For tracing the figure of the transverse velocity curves, the float course spacing in a canal should be: Symmetric about mid-channel; wide spaced over the level part of the bed; closer spaced with approach to the banks, and with one float course at the foot of each bank; closest spaced nearest the edge.

If discharge measurement is the aim in view, there should be a primary division into spaces as above, and these should be subdivided into a number of subspaces which should be multiples of 2, 3 or 6.

The properties of the average transverse velocity curves are as follows:

I. The velocity variation (in any one curve) approximates to the following distribution (in the case of a symmetric cross section with a level or wholly concave bed in a uniform long straight reach): the maximum velocity near the center; a very slow decrease of velocity from the center towards both banks, which becomes more rapid with approach to the banks, and is very rapid close to the banks; the curve is wholly convex

down stream, and is symmetric about mid channel.

II. Every marked change in the figure of the bed produces in general a marked effect on the figure of the velocity curve as follows: Increase of depth tends to increase of velocity, and *vice versa;* the maximum velocity line tends to be in the deepest channel (if sufficiently far removed from the banks); a convexity in the bed causes a concavity in the velocity curve, and *vice versa;* these effects are more marked in shallow than in deep water.

III. Velocity at same point in like curves increases and decreases *cæteris paribus* with rise and fall of water level.

IV. Like curves are similar under similar external conditions.

V. Like curves of equal mean velocity are *cæteris paribus* equally flat as a whole.

VI. Curves of low velocity are *cæteris paribus* flatter than those of like kind of high velocity.

VII. The flatness of a curve does not depend so much on the general depth of water as on the mean velocity, so that curves at low water are not necessarily flatter than curves of like kind at high water.

VIII. These curves are sharply rounded over sloping or stepped banks, and more fully rounded near vertical banks.

IX. At sites of similar character like curves are, each taken as a whole, flatter throughout at the wider sites.

X. Of unlike curves under similar external conditions in the same rectangular channel, the mid-depth curve is usually the outer (except near the center), the mean velocity curve intermediate, and the bed curve the inner.

Also, the mean velocity curve is one of the flattest and the surface curve is the most fully rounded.

The forward velocity near the edge decreases rapidly with approach to the edge.

At the edge the forward surface velocity is very small, perhaps zero. Near the edge there is a persistent flow, at and near the surface, from the edge toward the center, most intense nearest the edge, and decreasing rapidly with distance from the edge.

TRANSVERSE CURVES—GEOMETRIC FIGURE.

The investigation of the figure of the transverse velocity curve is a very delicate inquiry as the data available (velocity measurements) are not well suited to the purpose.

In curves of like kind, with same water level at same site, the velocities at same points are nearly proportional, so that such curves are approximately parallel projections of one another.

The transverse curves resemble semi-ellipses in general shape, but with flatness increasing as the water level sinks, in such a way that the exponent (of the order of ellipse) increases as the water level sinks.

The wet border (including the air in this term) is the ultimate source of retardation of flow.

Velocity at any point is probably a function of the average effective distance from the wet border.

AREAS AND DISCHARGES.

The number of repetitions of any observation measurement should be proportional to its "weight" in the computation formulæ.

The trapezoidal and arithmetic mean rules err in defect in the long run, and when the bed is concave, the error is not necessarily very small.

The chief advantage of the process of discharge measurement used in this work is the completion of the field work of a single result within a moderate time, within which the external conditions (except wind) may be presumed to have been tolerably constant.

The cubic discharge increases and decreases rapidly with rise and fall of water level, but depends to at least an equal extent on velocity.

The cubic discharge is sensibly constant from instant to instant.

A cross wind is liable to cause excess or defect, according as it blows to or from the gauge.

The chief source of the variability of cubic discharge measurements (under apparently the same external conditions) is that, in consequence of the unsteady motion each single result is an imperfect one.

Discharge tables should be tables of double entry, showing both gauge-reading and surface slope or gradient as argument.

MEAN VELOCITY.

The arithmetic mean of velocities errs in defect.

The mean velocity past a transversal and the mean sectional velocity are less variable from instant to instant than most of the individual velocities.

The mean velocity past a transversal varies sensibly from instant to instant.

The mean sectional velocity is constant from instant to instant, and in a higher degree than the cubic discharge.

The chief source of variability of mean velocity measurements (under apparently similar external conditions) is that, in consequence of the unsteady motion each single result is an imperfect one.

There is a general sort of agreement in the variation of the mean surface and central surface velocities, and also in that of the mean sectional, central mean and central surface velocities; also of the quantity $\sqrt{RS}$ with the latter three (R being hydraulic mean depth, and S= local surface slope).

The mean surface and central surface velocities, and also the mean sectional, central mean and central surface velocities, and the quantity $\sqrt{RS}$ increase and decrease with increase and decrease of either hydraulic mean depth or surface gradient.

Surface velocity measurements, and therefore also discharge measurements depending on them, are liable to be under or over estimated in high or up-stream or down-stream wind.

The mean velocity measurement is only slightly, if at all, affected by up and down stream wind. Surface velocity measurements made in high up or down stream wind are quite unsuitable data for discharge computation.

The mean velocity measurement is not sensibly affected by a high cross wind, but is *attributed* to an abnormal gauge reading.

The ratio $c=V\div U_o$ increases in general with increase of depth and probably also with decrease of velocity or surface slope. (V=mean sectional velocity, U_o= mean surface velocity.)

The variation of the ratio between mean sectional velocity and central surface velocity is very obscure, probably in consequence of the disturbing effect of the wind on the surface velocity.

The ratio $V\div 100\sqrt{RS}$ increases and decreases generally with increase and decrease of hydraulic mean depth; depends also in some complex manner on the surface slope, and also on the nature of the banks and bed.

The form of the Bazin co-efficient

$$C=\left(\alpha+\frac{\beta}{R}\right)^{-\frac{1}{2}}$$ is defective.*

The mean velocity and cubic discharge measurements obtained by applying Bazin's co-efficient for reducing central surface velocity measurements to mean velocity are generally *under estimated.* The deficiency is so great that this co-efficient is of little practical use in earthen channels.

Bazin's relation (expressing ratio of mean sectional velocity to maximum velocity), $c=100C\div 100C+25.34$ is fundamentally incorrect as a relation between mean sectional velocity, central surface velocity and C.

Kutter's co-efficient is one of pretty general applicability. When the surface slope measurement is a good average, it gives results whose error will probably seldom exceed 7½ per cent. in large canals.

The Mississippi Experiments Formula is useless as a *general expression* for mean velocity (except, perhaps, in cases of very low surface slope).

Further experimental research for an improved mean velocity formula is an almost hopeless task until the *proper* functional form is suggested by an improved rational theory.

A close approximation to mean velocity is more likely to be obtained by formulas depending on velocity measurement than on surface slope measurement.

Central mean is to be preferred to central surface velocity measurement for use in approximating to mean velocity.

The connection between mean velocity and any other primary velocity is of a more intimate and simple kind than between mean velocity and surface slope; the former being probably a more geometrical, while the latter is a physical relation.

For rapid approximation to mean velocity a *good* average central mean velocity measurement is (at present) the most reliable.

DISCHARGE VERIFICATION.

For comparison of successive discharge

* C is the ratio $V\div 100\sqrt{RS}$; α and β are quantities supposed to depend chiefly on the state of the bed, and, to a small extent only, on the slope. Bazin gave values to α varying from .457 to .853, and to β from .405 to 3.5.

measurements *at the same site*, such as are done in immediate succession, are much the most suitable.

For comparison of the cubic discharges *through successive sites*, the field work should be either *simultaneous* or else *in same* body of water at all the sites.

Under favorable circumstances the process of cubic discharge measurement used on this work yields consistent results.

The discordance between successive comparable results may be expected to be seldom over 3 per cent. (when the conditions are nearly similar and the circumstances favorable.)

CURRENT METER WORK.

By use of a proper current meter-lift the following advantages are secured, viz.: Certainty of orientation and position; of gearing and ungearing the current meter; also measurement of forward velocity (besides some minor advantages).

By separation of the recording works and connecting them electrically with the fan, the following advantages are secured; reduction of disturbance of the water; certainty of gearing and ungearing; increased delicacy; saving of delay of lifting to road, and vision of the motion of an index copying the movement of the fan. The uncertainties attending these instruments, as at present made, are very great.

SILT.

There is no obvious connection between the velocity and silt-density *at different parts* of a site. The silt-density varies from instant to instant at one and the same point.

The silt density and silt discharge do not appear to depend sensibly on either the depth or velocity at a site.

The silt density and silt discharge in the Ganges Canal depend chiefly on the quantity of silt present in the supply admitted into the canal.

EVAPORATION.

The evaporation from (an evapometer floating on) a large still-water surface or on a river is much less than from a small vessel on dry land (from the liability of the latter to become superheated).

The evaporation from the Ganges Canal, near Roorkee averages about $\frac{1}{10}$ of an inch daily (out of the rainy season).

The evaporation loss is about $\frac{1}{180}$ part of the full supply of the canal (or about 10 minutes full supply daily.)

The foregoing summary is the briefest possible compend of the author's conclusions drawn from the work, the account of the details of which fills, including tables and plates, three royal octavo volumes.

EXPERIMENTS ON THE FLOW OF AIR THROUGH ORIFICES IN A THIN PLATE.

BY A. FLIEGNER.

From "Civilingenieur" for Abstracts of the Institution of Civil Engineers.

THIS is a supplement to the author's previous article on "The Flow of Air through well-rounded Orifices."* He has determined experimentally for six orifices of different size, *i.e.*, 3.17, 3.87, 5.11, 6.62, 8.68, and 11.36 millimeters diameter, the weight of air flowing through the same at various pressures, both inside and outside the air vessel. The orifices used were made in brass plates 12 millimeters (0.47 inch) in thickness, drilled cylindrically for about $\frac{1}{2}$ millimeter, and then conically enlarged towards the outside at an angle of 45°. The results of the author's observations are given in a tabular form. From these experiments he has, with the help of formulæ already given in his former article—and various assumptions the sufficiency of which he considers to be attested by the results — developed several new formulæ for the purpose of calculating with greater accuracy than with those hitherto employed, the weight of air flowing per unit of time through an orifice in a thin plate.

* *Vide* Minutes of Proceedings Inst. C.E., vol. liii, p. 295.

The author adopts in the first instance the formula

$$G=aFp_m\sqrt{\frac{2g}{RT_m}\left(\frac{\kappa+\nu}{\kappa+1}\right)\psi} \quad . \quad . \quad (4)$$

where $\psi=\left(\frac{p}{p_m}\right)^{\frac{2}{n}}-\left(\frac{p}{p_m}\right)^{\frac{n+1}{n}}$ . . . (3)

p being the pressure in the most contracted section of the out-flowing air jet, a the co-efficient of contraction, while the other letters have the same signification as in the previous article.

The influence of heat imparted from outside is neglected as being very small, in consequence of which $\nu=o$, while it is assumed that the internal resistance offered to the outflow of the air as far as the section of greatest contraction is the same as for a well-rounded orifice; with the help of these suppositions the limits between which lies the value of the exponent n may be determined; they are 1.37 and 1.41. The principal object of the author's investigations is to determine the value of a; this he finds to be variable, depending on the diameter of the orifice, and in a greater degree on the ratio of the external pressure to the internal ($p_o : p_m$); these conclusions are arrived at by substituting the experimental values of the various quantities in the formula

$$a=\frac{G}{Fp_m}\sqrt{\frac{\kappa+1}{\kappa}\frac{RT_m}{2g\psi}} \quad . \quad . \quad . \quad . \quad (7)$$

which latter follows from the necessary modification and transformation of the formula (4) above stated.

The particles of fluid flowing from the orifice to the section of greatest contraction converge towards the axis of the jet, and in so doing follow certain curves. Assuming the orifice to be circular, the author supposes a section of the jet made in a plane passing through its axis, and in this plane the trajectory (a curve intersecting each of the curves followed by the different particles in the plane at right angles) to be constructed, from one end of the diameter D of the orifice to the other. In consequence of their convergent direction the particles of fluid in a section through the jet, made on the surface formed by the rotation of the trajectory round the axis of the jet, have a component of velocity at right angles to that axis, and therefore a certain amount of *vis viva* in that direction; at the section of greatest contraction this has disappeared, having been spent in work, that is, transformed into pressure. By formulating this relationship between the *vis viva* in a given section, and the work done in the contraction of the jet, the author arrives at a form of expression for a; in order to do this he has assumed the trajectory to be a cycloid. Although this is subsequently shown not to be the correct form, the results obtained would have remained unaltered had another curve been assumed.

After further investigation as to the influence of the pressures on the coefficient of contraction a, the author develops the following formulæ:

for $n=1.37$

$$a=(1-2.8860\,D)(0.7880+0.1390\cos\frac{p_o}{p_m}\pi) \quad . \quad . \quad . \quad (20)$$

for $n=1.41$

$$a=(1-2.8860\,D)(0.7522+0.1318\cos\frac{p_o}{p_m}\pi) \quad . \quad . \quad . \quad (21)$$

With the aid of these formulæ and those expressing the relationship between $\frac{p}{p_m}$ and $\frac{p_o}{p_m}$, which are:

for $n=1.37$

$$\frac{p}{p_m}=0.2820+0.4891\frac{p_o}{p_m}+\sqrt{0.0632-0.2374\frac{p_o}{p_m}+0.2266\left(\frac{p_o}{p_m}\right)^2} \quad (18)$$

for $n=1.41$

$$\frac{p}{p_m}=0.2716+0.4962\frac{p_o}{p_m}+\sqrt{0.0650-0.2435\frac{p_o}{p_m}+0.2333\left(\frac{p_o}{p_m}\right)^2} \quad (19)$$

the weight of air G flowing through the orifice in a second may be calculated from the formula (4) previously given (ν being o).

As the calculations involved in the expression just stated are rather complex, the author gives for practical purposes a more convenient formula (arrived at by purely empirical methods) for the weight of air flowing through an orifice, of the kind under investigation, in one second:

$$G_o=(3465-10000\,D)F\sqrt{\frac{p_m^2-p_o^2}{T_m}} \quad . \quad (24)$$

the weight G_0 being in kilogrammes, the pressures expressed in atmospheres of 10,000 kilogrammes per square meter, and the dimensions in meters.

This formula may be used for orifices varying in diameter from 3 to 12 millimeters (0.11 to 0.47 inch).

In the tabular statement of results to which reference has already been made, the ratio of the values calculated by the above formula to the corresponding ones obtained by experiment are given, showing a close agreement.

THE STORAGE OF ELECTRICAL ENERGY.

From "The Engineer."

A GREAT deal has been written lately concerning the storage of electricity by means of Faure's secondary battery. That M. Faure had succeeded in improving upon the Planté pile has been known for some time in scientific circles, but no undue importance was attached to the fact. Of late, however, most extraordinary stories have been set on foot; and the columns of the daily press have announced that steam is in a fair way to be superseded; that the day of gas lighting is all but over, and that the ship of the immediate future will be propelled by electricity. We have contented ourselves up to this moment with an announcement of the nature of M. Faure's invention, and we have refrained from doing more lest we should inadvertently lend the invention a notoriety it does not deserve. M. Faure's patents have been bought in France, and there is good reason to believe that a company to work them has already been formed there, and that an attempt to form a similar company in this country will soon be made. The most exaggerated claims have been put forward for the invention—claims so exaggerated that there was reason to think that no one would be foolish enough to believe them. Unfortunately, however, Sir William Thomson unwittingly wrote a letter to the *Times*, which appeared on the 9th inst., and which might easily be used with great effect by the promoters of a company. Sir William Thomson told the world that he had carried a million foot-pounds with him stored in a space of one cubic foot. This sounds very large, and the daily press in certain cases became more enthusiastic than ever. But large as "a million foot-pounds" sounds, it is really very little, and Sir W. Thomson has, we feel certain, been misunderstood, and his incautious words have been made to imply what he never meant them to convey. Before going further we may explain that the force stored up in Sir W. Thomson's "box of electricity" would not drive a 1-horse power dynamo machine for more than half an hour, even if all the energy it represented could be made available in producing mechanical work. We may further explain that as a matter of fact there is no electricity whatever stored up in the box, which contains neither more nor less than a powerful galvanic battery, the duration of whose action is short.

As the claims of the Faure battery to notice as a money-making invention are being freely discussed, we propose to explain what it is in popular language—language which will not, perhaps, please the electrician, but will, nevertheless, convey to the reader who knows little or nothing of electricity a good idea of the action of this wonderful box, about which so much is being said. Probably all our readers know that if a plate of zinc and a plate of copper be immersed in a weak acid solution, and kept from touching each other, little or no action will take place. If, however, a wire be used to connect the two plates, what is called galvanic action takes place. A current of electricity passes through the wire, and the zinc becomes rapidly oxidized. After a time the zinc will all be oxidized, save a small portion inside, which is protected by the oxide from contact with the acid. When this happens, the action of the battery ceases. Now let it be supposed that a powerful current of electricity from another battery is driven in an opposite direction through the first-mentioned battery; the effect would

be to undo what had been done, the zinc would be deoxidized and ready for work again, and if metallic contact were prevented, the battery might be kept ready for action for a considerable period. In practice the result we have indicated cannot be secured with zinc and copper, for reasons which we need not stop to explain. We have stated that it would, because the statement conveys a clear idea of what takes place in the Faure battery; for although deoxidation of zinc cannot be effected practically in the way indicated, it is possible so to act on an oxide of lead as to produce an analogous effect. The Faure battery consists of a strip of lead covered with red lead Pb_3O_4, the red lead, which is an oxide or rust of the metal, being kept in place by a sheet of felt. Two such lead plates having been prepared, they are coiled up together like the spring of a watch. This couple is then immersed in an acid bath and a current of electricity from a dynamo machine is driven through it. After two or three repetitions of the process, it is found that the red lead coatings on the two plates have undergone chemical change, one is converted into peroxide of lead, PbO_2, while the other is converted into metallic lead. By establishing conditions under which the peroxide loses some of its oxygen and the metallic lead gains some, a current of electricity can be obtained at once. Such, in a few words, is the principle of the Faure secondary battery. It contains no electricity, but its parts are in a condition to produce a certain quantity of electricity at will, just like any other battery. The peculiarity of the Faure battery is, it is said, that the current produced is powerful, very large in quantity, and high in potential. But this may be said of many other batteries.

As to the value of such a battery as a mechanical agent, we have to bear in mind first that it creates nothing. It can only give back a portion of the energy expended in producing the chemical change in the red lead, and there is every reason to believe that if employed to turn the best dynamo-electric motor to be had, not more than 50 per cent. of the original energy would be given back. Thus, instead of 1,000,000 foot-pounds in a box of one cubic foot capacity, we should have but 500,000 foot-pounds effective. But the great claim persistently urged for the Faure battery is involved in the idea that it represents vast concentrated energy—that nothing to parallel it in this respect is to be found in the whole world. Thanks are due to Professor Osborne Reynolds, of Owen's College, who lost no time in replying to Sir W. Thomson's statements. "The means of storing and re-storing mechanical energy," writes Professor Reynolds, "form the aspiration not only of Sir William, but of every educated mechanic. It is, however, a question of degree—of the amount of energy stored as compared with the weight of the reservoir, the standard of comparison being coal and corn. Looked at in this way, one cannot but ask whether, if this form of storage is to be the realization of our aspirations, it is not completely disappointing? Large numbers are apt to create a wrong impression until we inquire what is a unit. Eleven million foot-pounds of energy is what is stored in 1 lb. of ordinary coal. So that in this box, weighing 75 lbs., there was just as much energy as in $1\frac{1}{2}$ oz. of coal, which might have been brought from Paris or anywhere else in a waistcoat pocket, or have been sent by letter."

Professor Reynolds here hits the precise point which required hitting. The Faure battery is so far from being a unique and extraordinary storer-up of energy, that its powers are really very insignificant as compared with other arrangements. For example, a cubic foot of water weighs $62\frac{1}{2}$ lbs., and so is 12.5 lbs. lighter than Sir William Thomson's box, although of the same dimensions. To impart 1,000,000 foot-pounds of energy to a cubic foot of water it is only necessary to raise its temperature 20.7°. Professor Thomson may say that the work cannot be rendered available for the production of useful effect, and he is in a sense quite right; but so far as the bare statement that the Faure battery of one cubic foot contains 1,000,000 foot-pounds of energy is concerned, the assertion that a cubic of water when heated 20.75° hotter than it was before, contains 1,000,000 more foot-pounds of energy, is equally true. Again, a cubic foot of air at a pressure of 500 lbs. on the square inch, or 34 atmospheres, contains very nearly 250,000 foot-pounds of

energy, and this in a most convenient form for re-appearing as mechanical power. Owing to the cooling which it will undergo in expanding, a certain loss of useful effect will be incurred, but the compressed air will not lose as much as the Faure battery. It is true that a vessel of four cubic feet capacity would be required to carry 1,000,000 foot-pounds in this way, but the engine and reservoir taken together need not be larger than the Faure battery and its dynamo machine taken together. We might, were it necessary, easily extend this list, and show that the Faure battery has really no claim to be regarded in its present form as much more than an interesting scientific toy. Twenty feet of coal gas will produce an indicated horse power per hour in a good gas engine. There is no trouble whatever in compressing 20 cubic feet into a space of 1 cubic foot. If any one will take the trouble to carry a cylinder full of gas thus compressed from Paris to Scotland, he will be in a position to boast that he has done just four times as much as Sir William Thomson. That gentleman has carried 500,000 effective foot-pounds in his box; but 20 cubic feet of gas condensed twenty times represents 2,000,000 effective foot-pounds. All that Sir William Thomson can do with the Faure battery can be done with common coal gas. We do not know what it costs to charge a Faure battery, but we venture to think that coal gas will prove very much cheaper.

ARCHITECTURE GOVERNED BY TECHNICAL PRINCIPLES.

From "The Builder."

ARCHITECTS must always acknowledge technical rules to a far greater extent than painters and sculptors. These may, indeed, study with advantage certain principles of composition as practiced by old masters, and they would be none the worse for some knowledge of pigments, and such mechanical details of their art as our Professor of Chemistry can supply. Manual dexterity may also be increased by practice in the schools, but when all this has been done the artist must be left to himself, and his own innate feelings will be his best instructor.

The architectural student, on the other hand, is only on the threshold of his career when he has learned as much as this. He must devote himself to his art as to a learned profession, and though he must base his present designs and future expectations on the achievements of the past, he must study the existing needs of his own time that he may satisfy them, both with art and with common sense. Less unfettered than his brother artists, he has to deal with more intractable materials (to say nothing of more troublesome persons), and attempts at originality prove, in his case, too often to be a snare. It is, therefore, essential for him to deduce, if it be possible, from experience some assistance for his own practice, some principles which may aid him, not so much in imitating successful works as in avoiding errors.

An architect in designing a work of art has always to keep before his mind the *size* and *scale* of his building, the necessity of *stability*, and the method and materials of *construction*, all of which may be described as technical principles of architecture.

In discussing such principles, we will first refer to the element of size. In this respect architecture has an advantage over the sister arts; for the impression caused by the size of an architectural work is, to a great extent, if not altogether, independent of the art displayed in it.

We all know the effect produced upon us by a vast multitude of persons assembled in one place, though we may think little of the individuals composing it. We see a great gathering composed, it may be, of those who seem to us very ordinary and common-place people; we should not perhaps think of submitting our own judgment to any unit of the mass, but size tells, and the feeling has found expression in the saying, *Vox populi vox Dei.*

* A lecture before the Royal Academy, by the late Prof. E. M. Barry, R. A.

A large building will, in like manner, always produce an effect *per se*, and so the quality of mere size is a powerful influence in the hands of the architect. It is, no doubt, one of the lower attributes of architecture, and may to some of us seem of little importance in comparison with the artistic aspect of her work; but the fact, nevertheless, remains, that size is one of the chief elements of architectural impressiveness.

It is usually at first sight that size affects us, as it arouses interest by arresting the attention. If a traveler, entering a foreign town, catches a hasty view of two buildings, a large and a small one, it is to the former that he will first turn his steps. A small work of architecture, however beautiful, will not impress the mind of ordinary observers as much as a building which, from its greater size, is able to multiply parts—it may be, less perfectly designed.

As an illustration of the effect of mere size without any architectural design, we have the Pyramids. Seen from a distance, they are altogether uninteresting, and even when approached, display no beauty of any kind. The form, moreover, with its receding apex, is not favorable to the display of size. The visitor finds, however, that the latter quality grows upon him, and discovers with surprise that the outline which seemed so smooth from afar, is in reality notched with successive steps, and that each of these steps exceeds half the height of a man.

Here we have no architecture, but only a mere mass of large stones piled up in a symmetrical heap. Nothing more inartistic can be imagined, and yet because the heap is a big one, it has impressed, and will doubless continue to impress, the imagination of hundreds of thousands; extorting from them an admiration not to be obtained for structures far more interesting to the architect.

The worship of bigness is, in such an instance, reduced to its elements, and appears, in consequence, common-place and vulgar; but as the principle exists, and the architect has the power to turn it to account, he ought not to neglect it. And this, moreover, should be the case, whatever may be the actual dimensions of the work on which he may be engaged. There are few structures so small that they may not gain by the knowledge of the architect how to make the most of their size; and there can be no building so large that its importance may not be frittered away by injudicious treatment.

To obtain the due effect of size, the architect must study scale. He needs something to mark greatness by contrast. The Pyramids, seen in solitude, appear much smaller than when surrounded by life, and marking their real dimensions by contrast with the Arabs swarming over and upon them. A colossal statue, in the same way, gives no idea of its actual bigness, until an ordinary human being is placed beside it. It then asserts its real importance, just as a great man stands out from among his fellows, because his actions are nobler and better than theirs.

The Greeks did not erect large buildings because they did not require them, and their temples impress us rather as beautiful shrines for sculpture, than as grand architectural monuments. The effect produced in the case of the Parthenon, for example, is by exquisite proportions combined with beautiful detail, and not by size.

The Roman architects, on the other hand, fully appreciated the value of this means of obtaining architectural effect. Large edifices, such as the Coliseum, were the fruit of their energies, and Rome, as the capital of the civilized world, became the center to which all turned in art, as afterwards in religion and literature. The Roman Empire, extending from sea to sea, gave security to subject races, and assured to them material prosperity. Architecture as an art of peace consequently flourished, and throughout the vast empire imposing buildings arose, such as temples, theatres and baths, while public works of utility, like aqueducts and roads, were not neglected.

The Coliseum impresses us at least as much by mere size as by the beauty of its parts. These, indeed, are in no way remarkable, although from their frequent multiplication they give a value to the whole structure, and supply the necessary measure by means of which its vastness may be appreciated.

The great Roman baths are in too imperfect a condition to enable us to speak confidently of their original architectural

effect; but from the remains that have been discovered, particularly of mosaics and sculpture, we have every reason to conclude that their vast size was not thrown away, as has been done at St. Peter's; but that details of moderate dimensions, and statues but little exceeding the size of life, gave that opportunity of comparison which is essential to the architecture of a noble building.

In the Middle Ages we find the same principle worked out, and with even more technical skill. The builders of our cathedrals did not, indeed, aspire to the reproduction of the broad surfaces and spacious vaults of Imperial Rome. They sought to produce architectural grandeur rather by a repetition of parts than by any single exploit. Their works contained details of moderate dimensions, often repeated. The effect of such multiplication of small parts is to give scale to the whole building, and to make the latter appear even larger than it really is.

I dare say some of you, in summer wanderings among the many architectural beauties of France, have suddenly come upon a great church huddled up among mean buildings, clinging to it like parasites. The first sensation is that of incongruity, to be deplored, and, if possible, removed; but reflection and observation will often convince you of the gain of impressiveness derived by the giant structure from the contrast with its pigmy neighbors. The means are, of course, not those which the architect would desire as the best, and it is his part to produce the same result by art, which has been here attained by accident.

We may observe such a result of purely architectural design in some of the great portals which are more the special glory of French than of English churches. In these instances we shall find the door itself to be of moderate dimensions, set as it were in a frame, or rather succession of frames, of great architectural elaboration. Order after order of columns decorates the splayed sides of the recess in which the door is sunk, and between the columns, and in the arches over them, an infinity of small figures and canopies fill the whole surface with details on so small a scale, that they invest the whole composition with an air of size and grandeur unattainable by any other means.

I am now speaking of such designs only as illustrations of the principle under consideration, and apart altogether from the artistic value of the means adopted to carry it out; for, however great may be my admiration for the designs referred to considered as a whole, I have never been able to reconcile myself to small figures tilting forwards in the arches, so that they would fall on the heads of the entering worshippers if not artificially secured in their places by unseen supports. As an illustration, however, of the value of small detail in giving the effect of great size, I know of nothing better than these noble French doorways. If they fail in giving perfect satisfaction, the reason, in my judgment at least, is that they are apparently deficient in another essential principle among the technical rules of architecture—stability, to which we will now briefly turn our attention.

In a world full of turmoil and change, we like to look upon some ancient building, standing as a landmark in the midst of tumult—a rock beaten by the stormy waves in unavailing uproar. To satisfy this requirement, architecture should give us not only stability, but a stability which is obvious, even to the unlearned.

No one who enters Westminster Abbey gives a thought to the sufficiency of the columns and arches. This he is able to take for granted by reason of an apparent superabundance of strength; and though such an appearance may sometimes be misleading, doubts would only suggest themselves to a scientific inquirer, or one well acquainted with defects which may perchance lurk in the construction. It is different with some modern works, the offspring of science alone. Here we find light iron roofs and scanty columns, and wonder how long they will stand, and trust they will do so till we are far away. Or, we are called upon to admire the cleverness of the designer, who has used a less weight of metal than has ever been adopted before. In such cases, stability, in its true architectural sense, is altogether wanting; and when we find ourselves marveling at the mechanical ingenuity of the work, we may be sure that such ingenuity has banished art.

In structures in which the horizontal principle of construction has been fully

carried out, as in Egypt and Greece, the distances between the columns were small, though, as we know by the obelisks and other things, stones of large dimensions could have been procured. The quality of stability was here assured by the obvious preponderance of the supporting element of the design, other considerations, whether of art or utility, being sacrificed to this principle.

From the examination of the past as well as from our own common sense, we may deduce the conclusion that, in architecture, we must insist on stability as an essential principle, not only of necessary safety, but also of art. It is, of course, a common-place that an architect must not allow his buildings to fall down. If he did, he would speedily find himself within the strong grasp of the law. But it is not always equally recognized that, as a question of art, a piece of architecture must look stable, as well as be stable.

The Parthenon not only stands on a rock, but has a plain and satisfactory base line in the steps which surround it. The Romans followed the same rule, or, when they departed from it, placed their columns on a stylobate, which gives in another way the same appearance of stability. Mediæval architects did not attach the same importance to an unbroken base line which it would have been, indeed, difficult for them to have obtained, in consequence of the bold projections of their buttresses and turrets; but they were careful to strengthen the lower parts of their buildings with projections from the general line of the work, and plinths, often doubled and trebled.

It is difficult to overstate the artistic value of a good base line. There may be instances in which great vigor may be sought in height, as in the case of certain forms of towers, such as that of the great Campanile at Venice. A tower thus standing alone makes, perhaps, the most of its height by starting abruptly from the earth. It almost seems to rush upwards from the ground as a rocket might do, without a note of preparation. But setting aside exceptional cases, stability in architecture requires that a building should appear to rest on a secure foundation, and for this purpose demands a good base line.

Common sense tells us that a wall should be stronger and thicker at the bottom than the top, and what is true of the individual wall is also true of the structure as a whole. Architects are required by law in our great towns to widen their walls downwards, and, moreover, to add to their footings, which may even double their thickness, so that walls 3 ft. thick may start with a width of 6 ft. at their first commencement. Where basements exist all this is below ground, but when we approach a building the eye gives no credit for such hidden construction. The building, as far as the spectator is concerned, rests upon the surface of the earth, and unless the design provides in some manner for a satisfactory base, he is conscious of a want in the absence of that obvious stability which we hold to be a rule of art.

We may see the effect of such a base as I have been discussing at the Houses of Parliament. Here we have a building of great elaboration erected upon a plain granite basement, the latter being at times concealed from view by the rise of the tide. The building, owing to the unfavorable level of its site, is not seen to advantage from Westminster Bridge, and can be far more advantageously inspected from the opposite bank of the river. From this point of view the artistic value of the base line can be thoroughly appreciated, and the contrast observed between its presence and absence according to the fallen or risen tide.

The absence of solidity is only too painfully evident in much of the street architecture of modern days. The architect who is called upon to design such work is not to be envied, for under the pressure of commercial requirements, he will too often find it impossible to secure apparent stability in his work, as long as shops exist, and demand great areas of plate glass in their windows, hiding the necessary supports in the shape of some miserable iron bars behind goods or mirrors, which serve to conceal from view their stinted proportions.

I do not suppose that one person in a thousand, who uses these shops, gives a thought to their instability, notwithstanding the warnings which now and again have been given by the sudden fall of such buildings, carrying too often death and destruction with them.

The problem appears hopeless of solu-

tion as long as the conditions of it remain the same. Possibly changes in the current of trade, like those which are now leading to the concentration of business in large establishments, may tend to diminish the importance of the plate glass window in the eyes of the shopkeeper, and so relieve the architect of some of his difficulties.

The French method of laying out blocks of buildings with inner courtyards, approached by covered carriageways with handsome entrances, possesses, independently of convenience, great architectural advantages over our own system of unbroken frontages of plate glass, forming the ground story of our streets.

Architects cannot, however, enforce changes of this kind, however much they may desire them, and in the meantime, all that seems feasible with us at present is to group the upper windows in such a manner as to avoid, as far as possible, the appearance of great central weight, and to make the most of such solid piers as are still permitted or enjoined by the law, in the interests of public safety.

Passing now from the principle of stability, we come to the question of construction, which is, of course, intimately connected with it. It is here that the architect feels in its full force his lack of freedom in contrast with the liberty of his brother artists. While they do pretty much as they like, he must bear a heavy responsibility. His drawings may be as good as theirs, but, unlike them, they are not final, but only a means to an end. It is his duty not to make pretty drawings, but to erect good buildings. After the spirited sketch comes the matter-of-fact builder, who asks how it is to be carried out—to be followed by the estimator, whose dispassionate calculations often chill the enthusiasm of the most hopeful architect, and send a shiver of economy through his system.

If knowledge of construction is thus essential to the architect on the utilitarian side of his art, it is not less necessary in an artistic sense. Not only is it important to know in what positions of a design certain materials may be safely used, but also what will be their appearance and effect. The former requirement applies to the scientific, the latter to the artistic element in architecture. Designs which may be effective in one material will be unfitted for others. Faults of execution may interfere with well-studied detail. Color may be well or ill introduced; magnificence or meanness of effect may be produced by the dimensions or quality of the materials used.

These are all questions of construction, and thus construction becomes one of the leading technical principles which affect architecture. The materials now at the disposal of the architect are, for the most part at least, the same as he has used for centuries. Their qualities have been duly tested, and the limits of their use, with some exceptions, ascertained by actual experiment.

In modern times, the use of iron in construction has, however, been so greatly extended as almost to invest it with the character of a new material, chiefly employed by engineers and shipbuilders rather than by architects; for we cannot rank such buildings as the Crystal Palace among works of architecture. The increased employment of iron has, even at present, added to some of the architect's difficulties, as in that of the top-heavy street architecture to which I have just referred.

Before iron beams were to be readily obtained, the architect could work on the old method of making the walls in the upper part of a building rest upon those of the lower stories. Any reason is now considered sufficient to justify departure from this sound principle of statics, and if the architect venture to remonstrate, his scruples are at once set aside; for is not the iron worker at hand, with his sections of all shapes, sizes, and calculated strength?

I have just referred to the Crystal Palace, and it may, perhaps, be as well to mention it again, as it is the most conspicuous instance we can desire of an iron and purely constructional building. It is not architectural, for it does not comply with the essential principles of architecture. It certainly possesses size, and to this element of impressiveness it owes whatever success can be claimed for it. Stability, in the sense we have been considering, is not attained, and the construction is throughout that of the conservatory building engineer, whose science has been at work, paring down columns and girders to the minimum of necessary strength. Construction is here

all in all, not as we have seen it should be, only the assistant or handmaid of architecture.

I remember when we were told that the Crystal Palace had inaugurated a new era, and that it was only stupid people like architects who could doubt for a moment that our architecture would thenceforth take a new departure. It was useless to point out to a public enamored of a new toy, that not only in stability would such buildings prove defective, but that they even failed from the first in their primary duty of affording shelter.

The architect, accustomed to such permanency in his work, could not regard with favor a so-called system which, by the expansion and contraction of its materials, seemed to its admirers about to establish the principle of perpetual motion, as a novel technical rule of a newly-invented art.

If architects had originally any reason to feel aggrieved at the manner in which their ever-ready teachers assumed their usual position of superiority, the "magic of patience" has shown them that, notwithstanding all glowing predictions, the Crystal Palace has found few imitators, while its unfortunate shareholders have had but too much reason to ponder over the question of stability, in connection with the cost of repairs. From this and other specimens we do not, it must be confessed, derive much encouragement to expect new and speedy developments of iron architecture.

THE SECONDARY STRAINS IN IRON STRUCTURES.

By DR. E. WINKLER.

From "Deutsche Bauzeitung" for Abstracts of Institution of Civil Engineers.

THE calculation of strains in triangulated systems (girders) is generally performed as if the bars were joined at their ends by hinges offering no frictional resistance; so that, when the system deflects in consequence of the lengthening and shortening of the bars, and the angles of each triangle change accordingly, the bars retain their condition of being straight. This is not the case in reality, especially in girders with riveted connections. The bars are compelled on the one hand to retain their angles towards each other at the junctions, and on the other to follow the altered positions of the corners of the triangles, the consequence being the bending of each bar with generally one point of contrary flexure, and the occurrence of transverse strains (*secondary strains*) in addition to the axial strains (*primary strains*).

If S be the axial or primary strain in a bar, F its sectional area, J its moment of inertia, v the distance of an extreme fiber from the neutral fiber, and M the bending moment, then N, the maximum strain, is:

$$N = \frac{S}{F} + \frac{Mv}{J} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If further a be the length of one side of a triangle a_1 and a_2 the projections upon it of the other two sides σ, σ_1, σ_2, the rates of changes of length of the corresponding three sides, h the height of the triangle measured from a, then the change of the angle opposite a is:

$$\Delta\alpha = \frac{1}{h}(a\sigma - a_1\sigma_1 - a_2\sigma_2) \quad . \quad . \quad (2)$$

If finally τ is the angle which the end of a bar in the bent state forms with the straight line between the ends of the bar (*deviation angle*) so that τ_1 and τ_2 are the two deviation angles of a bar having a length $= l$, a moment of inertia $= J$, and a modulus of elasticity $= E$, then the moments at each end of the bar are:

$$\left.\begin{aligned} M_1 &= \frac{2EJ}{l}(2\tau_1 + \tau_2) \\ M_2 &= \frac{2EJ}{l}(\tau_1 + 2\tau_2) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The process of calculating the strains is now as follows: By means of equation (2) all the angles in the system after the deflection are ascertained, assuming that the bars remain straight; then, since the angles between the bars at their ends re-

main unchanged, there would, in the expressions for the deviation angles of the bars of the same junction, be only one unknown quantity, viz., the deviation angle (τ) of one of the bars. By applying these equations (3) to each bar at one point of junction, and considering that the aggregate of the moments for that point must be = 0, and, by doing the same for each point of junction, so many equations are obtained as there are unknown deviation angles τ. By putting these values into equations (3) all the moments are ascertained, and consequently by means of equation (1) the strains are found. This solves the problem. For the calculation of examples, equation (1) can be brought to the following form:

$$N = m \frac{v}{h} k \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where k is the primary strain, h the depth of the girder, v the distance of an extreme fiber from the neutral fiber, and m a coefficient dependent on the geometrical conditions of the structure. The formula establishes at once the rule that the bars in a girder should not be too broad. Calculating m for various examples, the author finds a number of results, some of which are as follows:

Warren girder—single system, broad flanges and narrow diagonals (*i. e.*, measured in the vertical plane); m (for flanges) = 2; m (for diagonals) = 6 to 12.

The same—narrow flanges and broad diagonals; m (flanges) = 1.0 to 1.4; m (diagonals) = 5 to 14.

Panel girder—single system, broad flanges; m (flanges) = 2; m (diagonals) = 8 to 14; m (verticals) = 17 to 20.

The same—narrow flanges; m (flanges) = 10 to 23; m (diagonals) = 1.7 to 3.1; m (verticals) = 3 to 5.

Putting m into equation (4) it is found, for example, that the secondary strains in the end verticals of panel girders may amount to 30 or 35 per cent. of the primary strains; and further, as a general result, that panel bridges contain greater secondary strains than Warren or lattice girders.

Lattice girder—parallel flanges, double system; diagonals free at crossings. This is generally under similar conditions as the girder with a single system, but at the ends the secondary strains amount to about 40 per cent. more.

The same—diagonals rigidly fixed. The secondary strains are in the center about 13 per cent., and at the ends about 70 per cent. of the primary strains. This is very much increased if only one system is loaded, viz., in flanges to the 3.5 to 12 fold, and in the lattice work to the 2.5 to 5 fold; but if additional narrow verticals are introduced, the strains are the same as if both systems were loaded.

Girders with polygonal flanges are advantageous, on account of the dingonals and verticals being narrow. The secondary strains in parabolic girders with single system amount to only 8 to 12 per cent.

Systems with hinged connections. Here is:

$$N = \tfrac{1}{2} f \frac{d v}{r^2} k,$$

d being the diameter of the pin, r the radius of gyration of a bar, and f the coefficient of friction. For rectangular sections and $f = 0.16$

$$N = 0.24 \frac{d}{v} k.$$

This applied to some existing bridges, gives secondary strains not inferior to those with riveted connections; but the advantage of pin connections is that the vibrations from the moving load probably overcome the friction to some extent, and that consequently the secondary strains due to the fixed load need not be taken into account. In bridges with riveted connection an artificial mode of erection would be required to produce a similar result.

THE *Archiv der Pharmacie* gives the following formula for making paper for wrapping up silver: Six parts of caustic soda are dissolved in water until the hydrometer marks 20 deg. Beaume. To the solution add four parts of oxide of zinc, and boil until it is dissolved. Add sufficient water to bring the solution down to 10 deg. Beaume. Paper or calico soaked in the solution and dried will effectually preserve the most highly polished silver articles from the tarnishing action of the sulphuretted hydrogen which is contained in such notable quantities in the atmosphere of all large towns.

ACOUSTICS IN ARCHITECTURE.

By A. F. OAKEY.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

OUR momentary dependence upon the sensations of our ears is perhaps only appreciated by those who have been deprived of hearing, whether for life by disease or accident, or for a time by the faulty construction of theaters, concert halls, lecture rooms and churches. The habit of not hearing, of not marking that Falstaff was conveniently troubled with when Justice Shallow demanded his £1,000, has to a great extent become the unaffected cause of somnolence in the occupants of pews, fatigue in playgoers and a lack of enthusiasm in musical audiences. A performance or a discourse must be powerfully interesting to hold the attention to the end, in an apartment where one is at times not quite certain that the sound has reached as it was delivered, and at times certain that it has not. After a few moments of this painful exercise one relapses into an indifferent state of mind, soon followed by sleep or irritable fatigue, neither of which conditions tend to increase our respect for the entertainment. We have all heard speakers and witnessed performances for the second time in a different place with very different feelings of pleasure, often modifying our opinions of the performance accordingly; and it can be shown that there are places of amusement in which no performance, however attractive in itself, has succeeded, and churches in which no preacher, however eloquent, can make himself felt. For the mysterious disadvantage in these cases all manner of causes are discovered; but eliminating those cases in which the disadvantge is thought to be one of location, of ventilation, or what not, we still have a number in which the disadvantage is plainly in the difficulty or impossibility of hearing distinctly, and without fatiguing mental effort.

To remedy this defect, and to discover a system by whose application to any circumstances we may be certain of acoustic success, has been one of the scientific aspirations for ages.

WITH THE ANCIENTS

the problem was a much simpler one than we have to solve, because all that they attempted or desired to achieve was the reinforcement of the voice in the open air, their theaters being only covered with a velarium when necessary, and the performers chanted their parts in a monotonous key to thousands of listeners, seated in rising tiers surrounding the stage on three sides, in rather more than a semi-circle. The necessary reinforcement to the voice was given by a very beautiful and simple expedient of three cones, placed in a central position so as to be in the same relative position to the player as he moved about on his semi-circular stage; there were bells hung over the cones, and so hung that they could be raised or lowered at pleasure, the cones filling more or less of the interior of the bell. These bells had each a tone peculiar to itself, the three together forming a chord, to include the compass of any recital, and by raising or lowering the bells they could be made to answer to all intermediate tones or semi-tones. In this way a resonance was produced that was always in unison with the voice and redoubled its apparent force. This expedient has been used in later years with some modifications, jars or vases of pottery being substituted for the bells and cones, and these vases were built into the walls and roofs of churches, always at certain angles with the position of a pulpit, so that a preacher should find it less difficult to make the echoes in his exhortations. But these experimente were found to cause more annoyance from reverberation, than was balanced by the resonance they gave the voice. Examples of this acoustic pottery are found in St. Peter's at Norwich, St. Mary's at Youghal, and in many old churches on the Continent; in the latter, the jars or vases often being placed under the stalls of the choir, with the idea that their sonorous vibrations would be more

easily excited and sustained by the vibrations of the wood of the stalls. In St. Clement's, at Sandwich, the jars are built into the north and south walls of the chancel, close under the roof plates, as if with the intention of sustaining the resonance of the roof timbers. But all these contrivances were found to be inadequate, and the innumerable phases of the sounding board began to make their appearance, every possible form, size and material being placed in every conceivable relative position to the speaker; but there are no instances in which the defects thought to be remedied by these contrivances have been more than partially cured, while, in many instances, these experiments have only made confusion worse confounded. Of course, in every difficulty, reasons are as plenty as blackberries, and every conceivable experiment was tried without ever striking at the root of the evil, until physiology came to our aid, explaining how and why it is that we appreciate the sensation we call sound. And it is only within the last fifty years that buildings have been designed and built with a view to acoustic properties from the first. There are a number of more or less plausible theories now under discussion in professional circles, and the object of this essay is to explain the one of which I am the advocate. It will be necessary for me to state a few

PHYSICAL AND PHYSIOLOGICAL FACTS

with which most people are already familiar, as a corner stone to my theory, but I shall be as brief as possible.

Sound is the sensation of the auditory nerve when the air surrounding us is set in motion with sufficient force. There are two kinds of sound, musical tone and mere noise; the first being the result of a regular movement of the air—a movement that strikes the drum of the ear at regular intervals, producing a continuous sensation or note of the scale. The second (noise) is an irregular movement of the air, a movement that strkes at irregular intervals, producing not a continuous but a confused sensation. All individual sounds are musical tones, and consequently all noises are made up of musical tones, but there are innumerable combinations of musical tones that are not noises, because in these cases the intervals of vibration of each of the tones bear a simpler ratio to the intervals of vibration of the other tones—a ratio so simple that it is at once appreciated, and a continuous sensation results; while in a noise the intervals of vibration of the tones bear no commensurate simple ratio, and a confused sensation results. In musical com binations of tones the commensurate ratio that the intervals bear, is always expressible by the numbers 1, 2, 3, 4, 5 and 6. The minor sixth employs 7 also. Beyond this our ears cannot appreciate the ratio and dissonance results. The notes of the scale are determined by the number of vibrations in any given tone peculiar to each; for instance, the lowest note of a seven octave piano-forte makes 16½ vibrations per second, and the highest 2112. Our ears can appreciate both lower and higher tones than these, but we have all felt the discomfort of the sensation produced by the lowest note of some church organ, seeming to make the whole building to tremble with its 16½ vibrations per second, while the notes having more than 4000 per second are so sharp as to be painful and even injurious to the drum of the ear.

In the construction of all musical instruments, the object aimed at is to generate at pleasure, in the instrument, any one of a scale of notes, and to so reinforce that particular vibration as to transmit it to the air with sufficient force to convey it to our ears. The object of having a number of instruments of different materials and different forms of construction, is to produce different qualities of sound. The number of vibrations only determines the pitch of the note, but the quality is determined by the kind of vibration. A piano-forte string may vibrate from a stroke of the hammer 132 times back and forth in a second, producing the sensation we call the middle C, and a trumpet may be blown so that the vibrations of the air are the same in number. The notes are then in unison, but we easily detect the difference, owing to the different materials and means employed, and the consequent different nature of the movements. My reader will ask what has all this to do with the acoustic properties of buildings? But I must dwell a little longer upon musical instruments, be-

cause upon the facts I have stated, and upon a few more, rests the theory I am going to explain.

Of all

MUSICAL INSTRUMENTS

the violin is admitted to be the most perfect; that is, those made from one to two hundred years ago by Stradivanus, Nicholas Amati, Julius Guananius and a few others. These instruments show the most scientific application of acoustic phenomena. For instance, every piece of wood, according to its length, breadth and thickness, will answer to some note of the scale; that is, fine sand or powder scattered over the surface will range itself in regular patterns when the wood vibrates in sympathy with a certain note, and these patterns are always composed of parabolic curves. The vibration of a string sounding any note describes parabolic curves on either side of its natural straight position. These facts are beautifully applied in the construction of the violin. The top table is composed of light porous deal, the vibrations of whose fibers are rapid and easily excited, and this top table is pressed and bent so that its convexity describes a parabolic curve, and exactly that curve which a string would describe in sounding the note peculiar to this piece of deal. Again, the bottom table is made on the same principle, only being of maple, a harder wood, and consequently less easily excited to its slower vibrations; that is, the note of the top table is of a higher pitch than that of the bottom table, and this difference in pitch was always one whole note of the scale. Now, let us examine the interior of the instrument and see what results from this disposition. The convexities of the top and bottom tables without become concavities within, and the sound is reinforced as by two parabolic reflectors with a common axis, so that this little instrument can fill a spacious hall with its sonorous pulsations, originating in merely passing a few stretched horsehairs across the dried entrails of an animal.

But these principles alone do not constitute a fine violin. Exhaustive experiment shows it all important that the interior space be neither too long, too wide nor too deep, and we find in the finest instruments that these dimensions always bear a simple commensurate ratio expressible by the numbers 1, 2, 3, 4, 5 and 6; in fact, the same ratio that exists between the tones of a consonant chord. All this nice adjustment is merely to the end that each sound as it is produced, may be encouraged and reinforced so that it may be heard at a greater or less distance; but it must not be altered in the least in its pitch or quality, or the instrument is a failure and utterly worthless. Then, what goes on *in* this little instrument, is really what we are striving for in our halls, theaters and churches. We want the air to vibrate in exactly the same way throughout. Hearing correctly is simply the movement of the air in force and rapidity, being exactly the same when it strikes the drum of the ear as it was when it received its first impetus from mechanical appliances, or a human voice.

AIR

does not transmit sound by one vast vibration in its whole extent, but by a number of particles, each vibrating in exactly the same way, and with the same number of oscillations in any given time. The first impetus from the initial vibration may be from 6 inches to 32 feet unchecked, according to its velocity, and according to the extent of the resisting body of air. But this first movement recoils after imparting its waning force to the next space, which, of course, will be smaller; and that again, the movement being passed on in this way theoretically for ever, and the whole combined movement or passing tremor is figuratively called a wave of sound. Now, it is plain that inasmuch as the extent of the body of air determines, according to the initial velocity, the length, breadth and depth of the first unchecked movement, it is important that the extent in any one direction should bear some proportion to the force of the movement in that direction. For instance, in a room ten feet high, ten feet wide and fifty feet long, a listener at the end would hardly appreciate the same sensations as a listener at one side who should be within ten feet of the sound produced. Of course a sound has more force in the direction it is delivered, but this force

ears a certain proportion to the force in other directions; as, for instance, the average speaker can be heard at a distance of 90 feet in front or in the direction he speaks, 75 feet on each side, 30 feet behind, and 45 feet vertically. Taking any three of these figures we have 90, 30, 45, or 6 : 2 : 3; 90, 75, 45, or 6 : 5 : 3; 30, 75, 45—2 : 5 : 3, or always harmonic proportions. These proportions must exist in the first movement, or first disturbance of the air, or these distances as given would be incorrect, and if these proportions are true for the first movement they must be so for every particle of air that passes the sonorous tremor on to the next particle in any direction. Let us assume that the first movement from the mouth of a speaker, or from an instrument, is in one instance two feet in the direction it is delivered, then applying the proportion or ratio already given, we have 2 ft. $\times$ 1′.9$\frac{1}{3}$″ $\times$ 1 ft., and the figures will always retain their ratio while they diminish for each succeeding space. But how can this natural proportion be maintained if the resistance of the air in these different directions is not this proportion? Supposing, as is often the case, that the speaker faces in the shortest direction? Then the resistance in that direction is much less than laterally and vertically, while the force in that direction is greater; consequently, before those persons seated near the side walls have heard anything, those seated beween the speaker and the wall opposite have not only heard but have received an echo from the wall behind them. If we suppose this defective disposition to be changed for

A HALL OF HARMONIC PROPORTIONS,

in which the speaker delivers his voice as he should in the length, we will be much better off inasmuch as the sounds will be appreciated at all points with the same intensity, but we still have to deal more or less with that opposite wall and its echoes. Now, inasmuch as a vibrating body of air is spheroidal, it is plain that the angles of an apartment, the angles of wall and wall, or ceiling and wall, are a disturbing element; but in curving them we must adapt a curve that will not plunge us into new difficulties. Apartments have been built with a semicircular end and a cove cornice of a quadrant, resulting, of course, in a mere whispering gallery; others, like the Albert hall in London, have been elliptical in plan, and with an elliptical ceiling, resulting, as a matter of course, in the most exaggerated reflections at either focus of the ellipse. In our violin there is no such difficulty because the curves are parabolic, and in such curves the reflections are always parallel to the axis. Applying this, let us suppose that our hall is planned in harmonic proportions—meeting our first difficulty. Then let us suppose that the wall opposite the speaker or orchestra is a very gradual parabola in plan, so gradual that the focus is very near the wall; and let the cornice and wall vertically be half this same parabola and the result is that there is only one point on the floor near the wall where there is a possibility of a reflection—that is, at the focus; for the end of our hall is formed by the parabola we have chosen, describing half a revolution on its axis. There are still many more difficulties that beset us, and not the least is the combined result of

THE UPWARD TENDENCY OF SOUND

in an apartment, and the absorbing surface an audience presents with their clothing. It is not because you are at a greater distance, in your back seat, from what is going on that you cannot hear well, it is because there is a quantity of material in dresses, coats, bonnets, &c., between you and the performance, and because the sound is rising with the exhalations of all these people until it passes far above your head, as you sit on a dead level with your nostrils presented to the entertainment in the vain attempt to see if you cannot hear. There is no disadvantage to a listener in being at the farthest end of an apartment if only he has nothing but the air, and pure air, between his ears and what he listens to. I say between his ears, because that average height must determine the difference between the level of his seat and that of his neighbors in front. In other words, there should be not less than six inches, and, if possible, nine inches difference in the levels of the seats, so that they may rise in a gradual line from the first to the last. Wherever this has been done, as in the lecture hall of the University of London, it has proved success-

ful. On the other hand, how many halls with level floors are not utterly bad acoustically, whatever other advantages they may have, and these rising tiers also prevent any possibility of echo. There remain three important points to determine: Ventilation, materials and the treatment of galleries, where they are necessities.

The purity of the air in an apartment is more important to our health and comfort than to our sense of hearing. The ventilation of an apartment only influences its acoustic properties in two ways: 1st. The currents of air, or the general direction of

THE SYSTEM OF VENTILATION

adopted, should be in the direction of the sound toward the audience, and the foul air exits should be ampler to prevent strata forming of a greater density and heat than elsewhere, as such conditions transmit sound more slowly, and retardation means confusion. Cross draughts, if only strong enough, will render all sound indistinguishable; so that we have only one system to depend on—that of introducing our fresh air heated to a proper temperature, at the end of our apartment opposite the audience, and introducing it at as low a level as possible, while our foul air exits should be in the riser of each row of seats, and the hot air exits as high up as possible at the farther end, thus creating an upward and onward draught in the direction we desire the sound to travel, while we insure to every one in our rising tiers of seats a constant supply of fresh air from in front. In regard to the

MATERIALS THAT SHOULD BE USED

in finishing the walls and ceiling of an apartment, no inflexible rule can be laid down. While we may be quite certain as to the best method in any particular instance, the peculiar properties of all available materials being well understood, our choice of a material must depend upon one fact that is seldom the same in any two apartments, namely, the relative capacity—that is, the number of cubic feet of space for each person. The most exhaustive experiment has determined that when the number of cubic feet of space for each person exceeds 195, it becomes necessary to adapt a resonant material for ceiling or walls, or both; that when the space exceeds 210, both walls and ceiling should be of wood, and the greater the space per head beyond this, the softer and more porous the wood must be, and the greater the necessity of perfect construction, so as to preserve the continuity of the fiber, and the thinner the wood employed should be, the necessity al\ ays increasing for an air chamber back of this wooden surface, as the proportion of space increases. In other words, the greater the proportion of space to the number of hearers the more the necessity of lining the apartment with sounding boards. On the other hand, when this proportion is less than 195 feet per head, it becomes necessary to present a repellant, hard and unsympathetic surface like plaster; and should the proportion become less than 150, stone or an artificial stone becomes necessary, and even an iron plate ceiling has been found advantageous, provided the metal is in small plates and sufficiently thick to be excited with difficulty. All these facts are equally true of the floor surface, but there we have more control, always being able to provide a space below, to leave the floor uncovered or to deaden it with carpeting, and fill up below with deafening. This question of material is exemplified by the violin, with its subtle distribution of hard and soft woods of varying thicknesses.

There are many instances in which galleries become a necessary feature in an apartment, whether the apartment is used as a lecture room, concert hall, theatre or church, and therefore it has become necessary to contrive

SOME MEANS OF CONSTRUCTING GALLERIES,

so that they may not, as they certainly are likely to, render useless the nicest applications of acoustic science. Persons seated under a gallery seldom or never hear distinctly, because the air under the gallery is not set in motion at the same time as that in the open space. It is a system apart, and only becomes affected after the body of air in the open space has committed itself to a certain form of vibration, while it affects the air in the open space, inasmuch as it is not an impervious body like a wall, but simply a more confined and a less proportionate

body from its form. No worse form for consonant vibration could well be devised than the space under the ordinary gallery with its ceiling slanting upward and backward to an acute angle at the wall line, while its natural disadvantages are generally much increased either by the accumulation of bad air in this confined space, or by a direct cold draft from behind, tending to check all possible influence from the sonorous tremor of the air in the main central open space.

I cannot see why all these difficulties cannot be obviated by a simple contrivance which, at the same time, abolishes another nuisance accompanying the usual gallery—that of the supporting columns, one of which, wherever your seat may be under the gallery, is pretty sure to range itself between you and what you are looking at or listening to. The contrivance I speak of is merely to construct a gallery upon a system of curved iron supports which should begin on the floor line against the wall, and with a gradual curve outward as they rise; any desired projection can be obtained and the construction rendered perfectly safer by a system of stays at various points and at various angles back to the wall. The curved supports or ribs would, of course, occur at regular intervals, and the spaces between them should be filled in with wood or plaster surface, always following the curve of the ribs, and thus forming a grand cove in compartments, giving an opportunity for decorative effect not presented by the wall and ceiling under the usual gallery. Of course the curve should be parabolic for the same reasons already given, and should bear the proper proportion to the main curves employed in the construction of the hall. This contrivance at once obviates the disadvantages of the usual form of gallery in seeing, hearing and breathing, while it can be made to simplify the ventilation by providing hot air exits through perforations in the upper part of each panel of the cove, and thus leaving the air in the gallery above as equable as elsewhere, instead of its being, as is usually the case, a mere stagnant strata of the heated air from below. These perforations in the cove properly connected with the foul air shafts of the building may be made to draw with certainty by the disposition of the chandeliers, which should be hung all round the house on the extreme projection of the cove, at regular intervals, so that the heat rising from the lights finds its escape through the piercings in the cove, causing them to draw. This disposition at the same time obviates the annoyance so constantly felt from a large glaring central chandelier.

Reviewing the facts as I have stated them, I can see no excuse for building an apartment so that its acoustic properties are not as much a matter of course as keeping out the weather. There can be no instance where the principles I have briefly described cannot be applied, whether the apartment be a theatre, concert hall, lecture room, church or court of justice. There cannot be an instance of an apartment whose acoustic properties are defective, however fundamentally, where the defects cannot with certainty be remedied. Other considerations for other parts of the building would sometimes have to be sacrificed, of course, where the defect was one of dimension in some one direction; as, for instance, the raising of the ceiling of the room would involve the sacrifice of the room over head, unless the roof were also raised. But of one thing we can be certain, that the defects of two apartments are never exactly the same, and the alterations, additions or appliances that serve our purpose in one instance, will, in all probability, prove useless in another.

THE ACOUSTICS OF BUILDINGS

is a science with which intention has nothing to do; each problem must be studied by itself, and every possible contingency must be weighed and settled by some application of the few simple laws we have to depend on. There are many apartments that are agreeable to a speaker and to his audience, while to a musician and his audience they are more or less annoying, and this is easily understood, because the vibrations produced by speaking are not of so prolonged or regular a nature, and consequently cannot cause nearly so much reverberation. But, on the other hand, there is no instance of an apartment where music is heard to advantage where a speaker does not find himself free from restraint, oppression or a necessity for exhaustive exertion. Therefore, our problem is

plainly to build our apartment, whatever it is intended for, as if it were a music room, and we are safe. It is no matter what the general form of the space we have to deal with may be, we can always increase or decrease some one or two dimensions until we can fix upon some unit of measurement that shall express the proportions in some harmonic ratio. If our apartment is of necessity 100 feet long and 50 feet wide, we can take 25 as our unit, and by making our ceiling 50 feet high we have the ratio 2:2:4; or assuming any two dimensions as fixed, say the height 50 and the length 100 then we can make our width 75, using the same unit, and we have 2:3:4; or, supposing the height and width fixed each at 50, then we can take 75 as our length and have the same ratio 2:3:4, only differently applied. It would be easy to show that harmonic proportions are always attainable even

IN ALTERING OLD BUILDINGS;

and these often present defects that almost incline us to think that their designers were perfectly conversant with acoustic law, and had ingeniously contrived to violate every principle.

I remember doctoring a church in which the pulpit was placed at one side, so that the speaker threw his voice directly against a hard finished wall opposite, and the ceiling was wagon headed, that is, rising from the wall at an obtuse angle and forming another angle at half its distance from the center, so that there were two rows of panels either side of the center forming the ceiling and forming angles with each other and with the walls; these surfaces were also plastered in hard finish, and, consequently, the sounds were reflected from the wall, and again from the ceiling in a most complicated way, causing three distinct echoes at certain intervals that interfered with and rendered the exhortations of the preacher more or less ambiguous to those within certain distances, while those who sat at either end of the church heard a great deal going on without being able to distinguish anything. The organ was placed in a gallery at one end, and found great reinforcement from the wagon-headed ceiling. On measuring this church I found that it would only be necessary to lower the ceiling 2 feet, alter the paneling to 3 panels on either side of the center, finish them in hard wood, and make them form angles with the walls and with each other, so that their surfaces were tangent to the parabola I had used as a basis. Then to move the pulpit to the end opposite the gallery, make the pews face the pulpit, and the thing was done. Since this alteration our clergyman has preached to the congregation, who before were never satisfied with any one for more than a few months.

Two years ago I found a room in the town hall of a village in Massachusetts, in which meetings of all sorts were held, musical entertainments given and church festivals celebrated. This room had serious acoustic defects which were the result of two causes: 1st, according to the theory I have explained, the room was too wide for its height and length; 2nd, there was too much repellant surface in the hard finish plaster for the capacity of the room. At this time I was engaged in experiments of all sorts that related to acoustics, and as the cheapest means of remedying the defects of this room I had, with the authorities permission, plain pine shelving built up to the ceiling on either side of the room, making the shelves the necessary depth to bring the room into the proportions of 3:4:5. I then persuaded the managers of the town reading room to use the shelves to stow away old files of newspapers and periodicals which gave me the absorbent surface I wanted, and now the room is admitted to be acoustically a success.

When we understand the origin of the sensation we call sound, and the properties of the materials used in building, the guarding against this or that special contingency becomes extremely simple. In the City of New York, and in any locality where houses are built with party walls instead of each house being self contained, much annoyance is experienced by the intimate knowledge one is forced to acquire of the neighbors' habits owing to the transmission of sound through the party walls, and this is so appreciable that in some houses I have seen it would only be necessary for the neighbors on each side to give simultaneous musical entertainments, not only to make sleep impossible, but even to oblige one to exert the voice to be heard in conversation.

In one instance I know that legal proceedings were contemplated to restrain the occupants of a young ladies' boarding school from playing upon fifteen pianos at once, all of which were placed against the plaintiff's party wall. Whether the law was found to cover such a nuisance I have not heard, but if the suit could be brought I think one's sympathies would be with the plaintiff.

This sort of nuisance arises from two causes—niggardly economy, both of space and money, and, secondly, ignorance of the nature of materials. Of course, if space were unlimited, a brick wall could easily be built whose thickness would render it impervious, but the problem is to build the thinnest possible wall that will carry itself and its load, and that will not transmit sound.

We know that almost any material is a better conductor than air, and that if we can provide a sufficient air space with comparatively non-conducting material on each side, no vibration would be transmitted, but we can do better by dividing our air space by non-conducting material. In fact, this disposition is forced upon us by our lack of room. The trouble arises from plastering directly upon the brick, thereby saving the lath and one coat of plaster, as well as one inch in the width of the rooms.

Supposing the wall to be only eight inches of brick, which is too often the case, the sound would be more effectually checked by furring (that is, nailing strips all over the surface) and then lathing and plastering, than by increasing the thickness of the wall to 12 inches with the plaster on the bricks; because with our furring, the vibration has to pass through the plaster, the lath, and air space, the brick, another air space, and again the lath and plaster; and to do this the vibration must be unusually intense and sustained. The inch in the width of the apartments, and a small amount of money is sacrificed by this preventive measure, but how much comfort is gained? There is still one minor, but very appreciable cause for the transmission of sound through party walls that I have not mentioned; the injurious practice of giving the floor joists a bearing in the walls, however thin the walls may be; in fact, some rows of houses could not stand till their roofs were put on if the walls had not this assistance. But it is easily seen that when the joists are let into a thin wall on both sides that there can be little to separate them, and that the continuous fiber of the wood passes a vibration so readily that in a row of five or six houses so built, a blow upon the side wall of the last house, on the line of a joist, would be distinctly heard on the wall at the other end of the row. This objection is easily overcome by a better though somewhat more expensive mode of construction, but one that does not increase the thickness of the wall. The joists being carried by a few projecting courses of brick on each side, so that the whole thickness of the wall separates the ends of the timbers.

To return to my first enquiries, I shall explain the peculiarities of one or two existing buildings in illustration of my theory. Probably the most successful building acoustically that was ever constructed was

THE THEATRE AT LYONS,

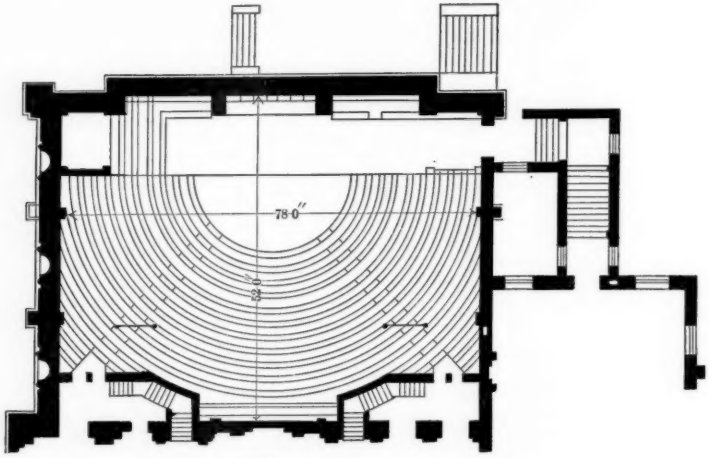
built by the architect, Sufflot, in 1754. I say was, because it was burned and reconstructed—burned in 1826 and rebuilt in 1831. As it now stands it is bad, acoustically, the owners believing that they could make some improvements in plan. The drawings of the original theatre, now preserved in Paris, completely corroborate the theory I have explained, while the theatre as it is violates it in several important particulars. The original theatre was in the harmonic proportion of 4 : 4 : 3. The present theatre is not in harmonic protion, and employs circular curves to a great extent.

The Albert Hall in London is an example in violation of my theory, and has been heavily draped to prevent the complication of echoes resulting from the elliptical form adopted. It is the largest place of amusement under one roof in the world, being 219 feet long by 185 feet wide, and 136 feet in height. A glance at these figures satisfies us that no commensurate ratio exists that is expressible in the numbers 1 to 6, and beside this fact, I have already mentioned the objections to the ellipse, where curves are employed.

The probable acoustic result of this place was pointed out by many acoustic

authorities in England before the building was begun; but as Captain Foulke, an engineer, had drawn up a description of the building, using the word "elliptical" in a prospectus, upon the strength of which subscriptions to the stock were received, it was found that any radical change would vitiate the preliminary preceedings, and so two million pounds sterling, more or less, were invested in a building which was certain from the outset to be a failure in the most important requirement.

The Free Trade Hall in Manchester is an example of a successful building, acoustically. The dimensions are 130x



PLAN

Fig. I.

78 and 52 feet in height, or with a unit of measurement of 26 feet; the harmonic proportion 5 : 3 : 2. The floor also rises from the platform to the rear wall, which is an exceedingly flat curve in plan. The exact nature of this curve I am unable to state; but from the published drawings it is not circular. The angle of wall and ceiling is also treated with a very gradual curve. There is a shallow gallery on two sides and one end of the hall, but the lobbies are placed under it, so that the objections that I mentioned to the space under a gallery are obviated.

IN THIS COUNTRY

we are not rich in acoustic successes, but the Boston Music Hall may be quoted as probably the best example we have, and this was built by the Boston architect, George Snell, who has adopted harmonic proportions, though the requirements did not admit a rising floor.

The Cincinnati Music Hall is considered by all musicians who have performed there to be at least as perfect acoustically as any American example. The interior of this hall was constructed from my plans, submitted in competition, and is—as nearly as the dual purpose of the building would admit—a realization of my theories as herein explained; length 200, height 80, width 120; or 2 : 3 : 5. Every room has a key-note—that is, there is always a note that creates more resonance in any particular room than any other note, and this fact should always be taken into consideration by a speaker or musician. If the key-note of a room is discovered. which is easily done, a speaker will find that his audience will hear better, and that he will find it less tiresome to talk in that key an octave above or below as best suits him. The old Salle de Concert in Paris is particularly sensitive in this respect, and has become like an old violin, being lined with wood that is always seasoning, and responds readily to the vibrations of instruments and voices.

Slight projections on walls and ceilings are in some instances advantageous,

where too much resonance is created; but this consideration is involved in determining the finish to be adopted. The object of a projecting cornice, or pilaster, may be to thicken the wall at that point, so as to check a continuous tremor passing along the whole surface, as is the case in a whispering gallery.

The first illustration is a plan of the lecture room of the University of London, which shows the disposition of an audience in a semicircle though in a rectangular room. This, room is found to be a success as a lecture room, and it will be noticed that on any radiating line from the speaker the distance is the same to the farthest listener, and that the proportion of length to width is 78 to 52. The cross section (Fig. 2) of this room shows the rising tiers of benches, giving each auditor the same opportunity, and the height of the room is again 52; recalling the dimensions of the plan 78x52 we have the ratio of 2 : 2 : 3, with a unit of 26. The longitudinal section (Fig. 3) shows the disposition of wall-surface which is ingeniously treated in plaster and wood, gaining a happy distribution of repellant and resonant material that has proved most satisfactory. This room has never, I believe, been used for music, and I am inclined to think that it would not be so successful as a music hall, because it is found that where the opposing wall is as near the singer or orchestra as the side walls, echoes or confused reverberation is apt to result from the reasons I have stated, especially if the wall is at right angles with the side walls; in other words, for musical purposes the sound should be delivered in the length, and should be met by a wall that is more or less curved.

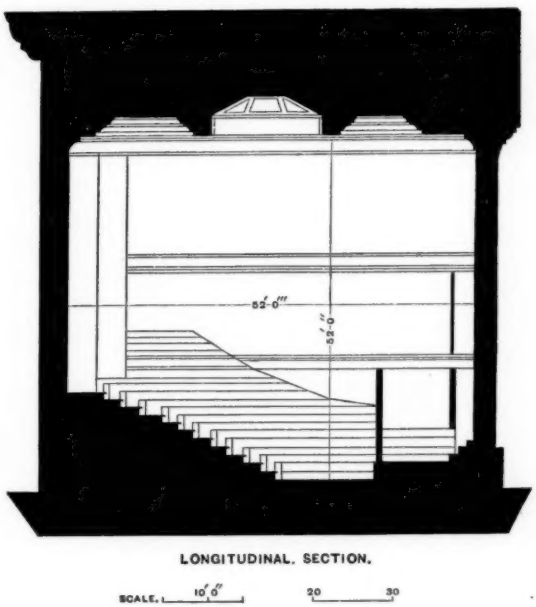
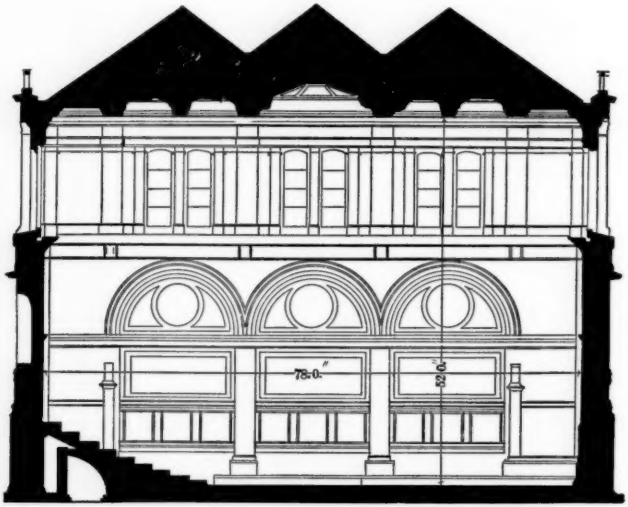


Fig.2.

Her Majesty's Theatre, Haymarket, (Fig. 4) is our bad example, and I need hardly explain why after what I have said. This illustration shows the plan as a horseshoe, 67 feet in length and 56 feet at its greatest width. The curve is circular. The section (Fig. 5) shows us an ingeniously bad contrivance for the

highest gallery, but the boxes are an advantage, as they tend to break up the surface and prevent the confusion that would arise from the form employed. The height is 57 feet, and with the dimensions already given—67 x 56—no commensurate ratio can be discovered. This theatre was always difficult to speak or sing in.

Figs. 6 and 7 illustrate my design for the Cincinnati Music Hall as I submitted it to the committee on preliminary studies; and Fig. 8 illustrates the plan of the hall as it was built. All the principles I have explained are embodied in this design.



TRANSVERSE SECTION
LECTURE ROOM

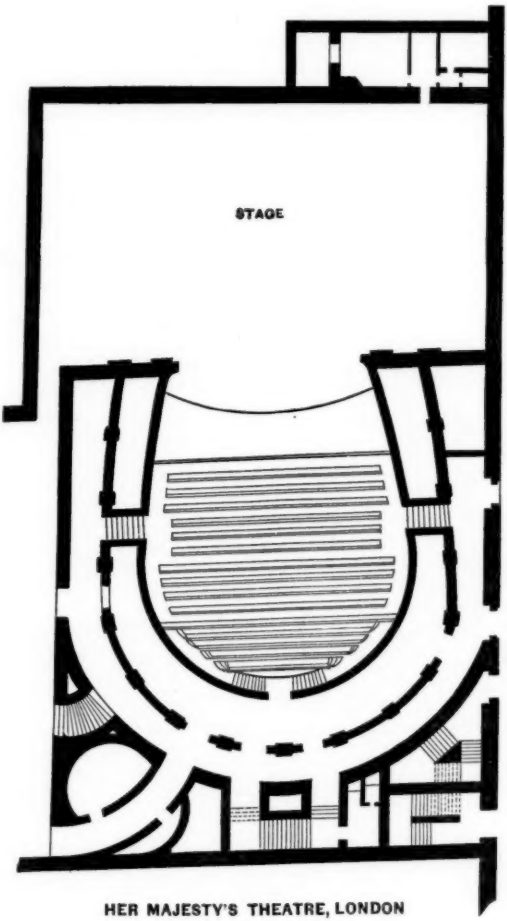
Fig. 3.

Figs. 8 and 9 illustrate a project which has not yet been executed, but which, as shown in plan and section, is at least apparently based upon plausible theories. The idea is evidently to make the auditorium a spheroid in form, and I am not prepared to say that there are not reasons for supposing that it might succeed, though I am inclined to think the diminution of vertical space at the further end a mistake, in so far as it relates to the ceiling. The rising tiers, in my judgment, sufficiently reduced the space to accord with the waning force and smaller disturbance; and it is curious to note that long before the publication of this design Mr. Furgusson, the historian, had touched upon the subject of acoustics in an appendix to his "History of Modern Architecture," in which he gives a small section of what he is inclined to think the best acoustic form for an auditorium, and his section is as nearly as possible the reverse of Fig. 9.

I regret that no discussion of the theories which led to the adoption of this form were published in explanation of the cuts, at least I have been unable to learn anything of such an explanation; but it is natural that a Frenchman should incline to accepting the authority of Savart, and of Lachez, rather than of Helmholtz, whose "Sensations of Tone as a Physiological Basis for the Theory of Music" is probably the most exhaustive and reliable work on acoustic phenomena extant.

There are not many books that will be found important in studying this question; but in addition to the above I name a few that I have read with more or less interest. It should also, in fairness, be said, that the interest a reader finds in any particular scientific work is apt to depend largely upon the corrob-

oration of his own theories it contains. I have tried to avoid this bigotry, and have in the foregoing set down as briefly as possible what I believe to be exact and trustworthy information for practical use. If I have at times seemed to take any important step for granted, it is only because my reader would hardly care to follow a laborious technical argument, which he can construct for himself by taking the same pains that I have not spared to become thoroughly familiar with the subject, and with all that has been published concerning it, even remotely, often reading a ponderous volume merely to satisfy myself that it afforded me no assistance. The appended list is a small library, but those works marked with an asterisk will, in my opinion, sufficiently cover the subject for all theoretical or practical purposes.



HER MAJESTY'S THEATRE, LONDON

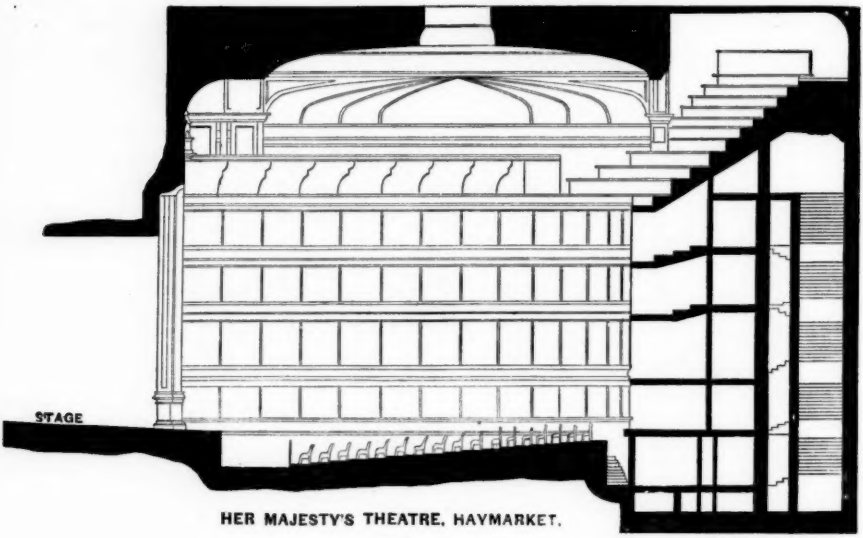
Fig. 4.

LIST OF BOOKS ON ACOUSTICS.

* "Sensations of Tone as a Physiological Basis for the Theory of Music," by Helmholtz. London translation.

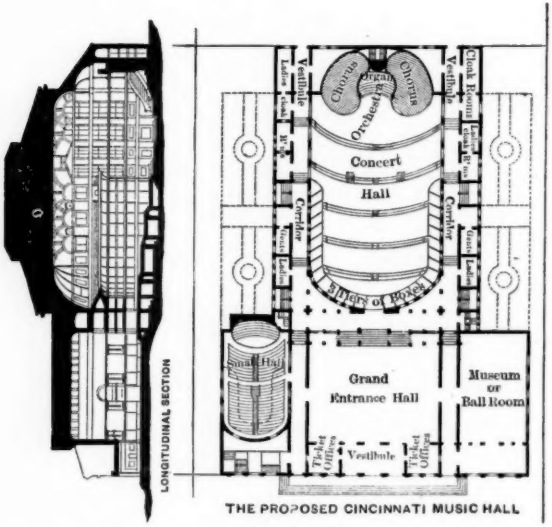
"Sound." Sir John Herschel. London, 1849.

"Memoirs de la Societé d'Arcueil." Vol. II. Biot, 1807.



HER MAJESTY'S THEATRE, HAYMARKET.

Fig.5.



THE PROPOSED CINCINNATI MUSIC HALL

Fig.6 & 7.

* "L'Institut, Journal des Academies," &c. Paris: Volumes for 1838–1840; "Cours d'Acoustique." Savart.

"Précis Elementaire de Physique." Biot. Paris.

"Cours de Physique," 7th edit. Pouillet. Paris, 1856.

* "Traité d'Acoustique." Chladni. Paris, 1809, and other works by Chladni in German.

*Papers contributed to the "Quarterly Journal of Science, Literature, and Art," 1827–9.

*"Report on Waves." Meetings British Association, 1843–44. Scott Russel.

*"Acoustique et Optique des Salles de Réunions Publiques." Theo. Lachez. Paris, 1848.

"Observations on the Principles of a Design for a Theatre." Wyatt. London, 1811.

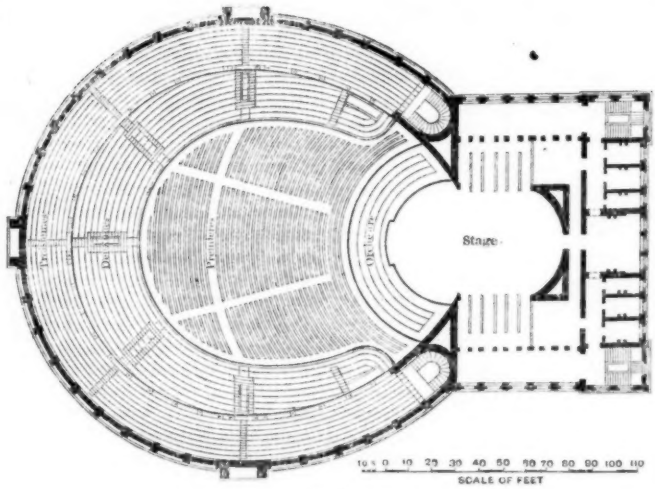


Fig. 8.

"Journal of Royal Institution," 1830–1831.

*Proceedings of Royal Institution."

*"Reports of British Association;" all at London, by Wheatstone.

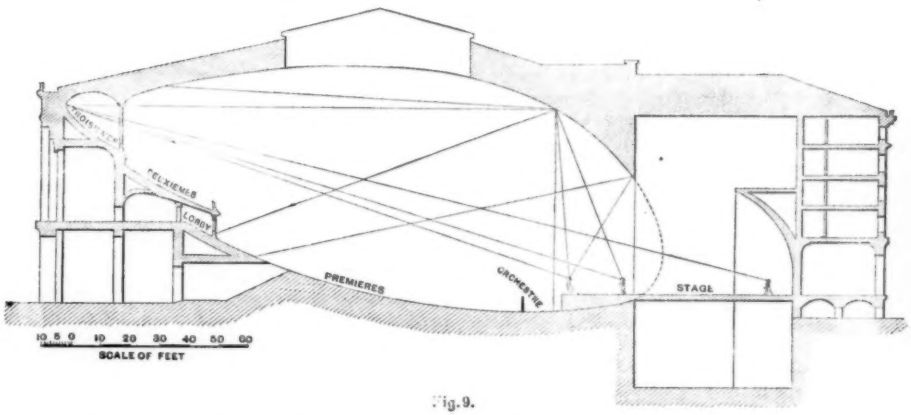


Fig. 9.

*"Annales de Chimie," &c.,'1816; papers by Savart, Wertheim, Lissajoux and others.

Arnot's "Elements of Physics."

"Sound." Brewer. London, 1854.

"On the Principles of Sound and their Application to the New Houses of Parliament." Webster, 1840.

"Observations on Sound." Matthews.

*Papers read before the Royal Insti-

tution of British Architects, in February, 1847. Scott Russell. See "Builder," 1847, pages 82-118; "Building News," 1858; Nov. 26, Dec. 3-10.

* "Description of a Parabolic Sounding Board erected in Attercliff Church." Pamphlet. London, 1829, by Rev. John Blackburn, M.A.

Paper read before Architectural Association, by T. Rodger Smith. See "Building News," Nov. 19, 1858. Another Paper by the same, before the Royal Institution British Architects, December, 1860. Printed in the "Transactions."

* "Acoustics in Relation to Architecture and Building. The Laws of Sound applied to the Arrangement of Buildings," by T. Rodger Smith. New edition. London: Virtue & Co., 26 Ivy Lane.

AN EXAMINATION INTO THE METHOD OF DETERMINING WIND PRESSURES.

By F. COLLINGWOOD, Member A. S. C. E.

Transactions of American Society of Civil Engineers.

About two years ago, the writer prepared some notes on the subject of wind pressures, which, upon consideration, he decided not to present to the society. Subsequent events have led to a modification of some of the views then held, and with these modifications they are now presented. On account of the inherent difficulties surrounding the subject, engineers have seemed to acquiesce in an empirical solution of the problem, and to decide that nothing better could be done. This is not in accordance with the spirit of investigation now extant; neither does it tend to economy or safety in construction; and it would seem that the time had come for a more thorough treatment of the matter.

In a paper by John Smeaton on the "Construction and Effect of Windmill Sails," read before the Royal Society May 31 and June 14, 1759, he gives a table of wind pressures, which have been quoted as authority in nearly all the hand and text books published since, and which is adopted by the United States Signal Service. He prefaces it thus:

"The following table, which was communicated to me by my friend Mr. Rouse, and which appears to have been constructed with great care, from a considerable number of facts and experiments, and which, having relation to the subject of this article, I here insert as he sent it to me; but at the same time must observe, that the evidence for those numbers where the velocity of the wind exceeds fifty miles in an hour, do not seem of equal authority with those of fifty miles and under. It is also to be observed that the numbers in column three (giving perpendicular force on one foot area in pounds avoirdupois) are calculated according to the square of the velocity of the wind, which, in moderate velocities, from what has been before observed will hold very nearly." (In a foot note elsewhere he explains that certain experiments by Mr. Rouse were made with windmills by a special machine constructed so as to move the wheel or sails, as a whole, through the air, and noting the weight lifted by the wheel with varying velocities of movement. The table is, therefore, apparently based on resistance by the fluid to motion through it, and *not* on impulse or pressure from the air. Whether these experiments are the ones on which the table is based is not stated, but may be inferred, although from what follows, it would seem that the table was computed largely).

Referring to Weisbach's Mechanics (Vol. I., § 390, Ed. 1848) the formula for the pressure tending to move a prismoidal body by the force of an unlimited stream of uniform velocity in which it is immersed is

$$P=(f_1+f_2)\frac{v^2}{2g}d.$$

Where P=pressure in pounds per unit of cross section.
v=velocity of stream in feet per second.
f_1=coefficient of pressure at up stream end.
f_2=coefficient of pressure at down stream end.
d=weight of a unit of volume of the water.

He says that where the length parallel to the axis of the stream is zero (corresponding to a thin plate) Du Buat & Duchemin found that the sum of the coefficients

(f_1+f_2) will $=1\frac{86}{100}$ or $P=1.86\frac{v^2}{64.4}d$.

For a length of

$\frac{l}{\sqrt{\text{section}}}=1$, the coefficient=1.47.

For a length of

$\frac{l}{\sqrt{\text{section}}}=2$, the coefficient=1.35.

For a length of

$\frac{l}{\sqrt{\text{section}}}=3$, the coefficient=1.33, &c.

And as the ratio increases the coefficient continues to diminish.

Taking the case where it equals 1.86 as being nearest to the windmill sails, and substituting 0.07925 pounds as the weight of a cubic foot of air, we have where P = pressure in lbs. avoirdupois and V = velocity in miles per hour,

$$P=0.00492\,V^2. \quad . \quad . \quad . \quad (1).$$

This equation gives results the same as those given by Rouse in the Smeaton tables.

Dubuat found that where the ratio $\frac{l}{\sqrt{\text{section}}}=o$ (or the case of a thin plate moved *against* water) the coefficient became 1.25, or the pressure is 33 per cent. less than where the water moved against the plate. This is given in article "Resistance" in Brand & Coxe's Cyclopedia as 1.43. In the same article a cylinder, with hemispherical end facing the stream is said to offer only half the resistance of the same with a plain end. Also that while the impulse of the stream against a plate 1 foot square gave the coefficient as $1\frac{9}{10}$, a plate with one-tenth of a square foot of surface gave it as only $1\frac{4}{10}$, or nearly twenty per cent. less. Hutton's theoretical deductions, as stated by Weisbach, for a sphere moving against air give the coefficient as increasing from 0.59 for a velocity of 1 meter to 1.10 for a velocity of 600 meters. In Hutton's mathematics the resistance to a spherical projectile at high velocities is given as 0.43 of that to a plain surface.

Of a hemisphere convex side forward... 40 per cent.
Of a hemisphere plane side forward.....101 per cent., &c.

In Appleton's "Cyclopedia of Applied Mechanics," 1880, is a table of pressures calculated by Mr. Alfred R. Wolff in which account is taken of the difference in density of the air at various temperatures. At a mean it differs but slightly from Smeaton's table, and would seem to be based on the same formula.

Prof. Cleveland Abbe in the article on "Wind," in Appleton's American Cyclopedia, Vol. 16, writes as follows:

"Maxwell (see proceedings of the Mathematical Society, 1870) has given theoretical formulas and curves showing the movements of particles of an incompressible fluid streaming past a moving obstacle; while Hagen (Berlin 1872), has experimentally investigated these motions. Thiesen (Wild's Repertorium, 1875) has made a careful study of the experiments of Hagen and Dohrandt, and established the rule that the pressure of the wind against an inclined rectangular plate is really very nearly proportional to the square of the velocity, and the cosine of the angle of incidence of the wind, while the absolute value of the normal pressure is as given by Hagen's observations. The latter physicist (Berlin, 1874) has embodied the results of very careful observations at moderate velocities in a formula, which converted into English measures is as follows:

$$P=(0.0028934+0.0001403\,p)SV^2. \quad . \quad (2).$$

By introducing the term (p) Hagen has expressed the fact that the pressure depends to a considerable extent on the *shape* as well as the surface of the resisting body."

In this formula P = the total pressure in pounds avoirdupois; p = the outline or perimeter of the exposed surface in feet; V = velocity in miles per hour; S = the area in square feet. The formula

applies to plain surfaces (of no considerable depth) placed normal to the incident wind, and with the density of the air corresponding to a barometric height of 29.84 inches, and a temperature of 59° Fah.

The results from equations (1) and (2), together with Smeaton's and those of Hutton, are given in the table on the following page.

All are taken for one square foot of surface. It will be seen that Hagen's are about one-third less than Smeaton's, and Hutton's about midway between the two.

The diversity of views among practical men in determining the measure of surface acted upon, the probable wind to be resisted, and how the wind acts, are well shown by the evidence given at the inquest on the Tay bridge disaster. These differences are greater even than those in the tables.

The London journal, *Engineering*, January 2, 1880, in discussing the accident, takes the lee girder as exposing one-half as much surface to the wind as the windward girder, and the whole vertical surface of the train also as acted upon; and deduces according to different supposed modes of failures pressures from $23\frac{1}{2}$ to $35\frac{6}{10}$ pounds per square foot, expressing the belief, however, that the maximum pressure did not exceed $25\frac{1}{2}$ pounds.

In subsequent numbers of *Engineering*, evidence is given as follows:

Mr. Allan Stewart placed the pressure at 75 pounds. Mr. B. Baker placed it at 15 pounds; he had experimented on glass in frames, and anything like 40 pounds per square foot would have destroyed the signal boxes. He had never seen a structure blown down which was capable of withstanding 20 pounds, and he looked upon that as the maximum pressure to be taken for structures of any kind. Fences capable of withstanding only 13 pounds had withstood storms in all situations.

He states that the French engineers assume $34\frac{6}{10}$ pounds as the greatest pressure a train can bear without overturning. One of their engineers had gone into an examination of chimneys, and had assumed an arbitrary figure of 55 pounds as the force they had to withstand, and this figure was now taken as the pressure against bridges, &c. The same has been taken by Rankine.

Mr. Baker said that in his own work he assumes 28 pounds, and varies the *factor* of *safety* with circumstances.

Dr. Pole and Mr. Stewart estimate the pressure required to overturn the bridge with no train upon it at $37\frac{4}{10}$ pounds, and with the train, at $34\frac{1}{8}$ pounds (also, $28\frac{1}{2}$ pounds as needed to overturn the lightest carriage in the open, and 33 pounds when containing eight passengers and shielded by the bridge members).

Mr. Law places the same at $36\frac{3}{8}$ and $32\frac{2}{3}$, respectively, and that 40 pounds were needed to overturn the lightest carriage in the train.

Prof. Airy states that during the gale of December 8, 1872, the greatest velocity recorded was 57 miles per hour, and he thought the pressure had reached 50 pounds per foot. The maximum pressure recorded at Greenwich was 40 pounds and then the instrument gave way.

He had estimated the pressure in the valley of the Tay at 50 to 100 pounds. "The valley runs deep, and would make a channel for the wind." He thought 120 pounds ought to be provided for, but this would allow a large margin for safety.

He did not know whether such pressures ever obtained simultaneously over so large an area as the Tay bridge. In a letter respecting the Forth bridge, he had stated "that over very limited surfaces and for very limited times the wind pressure was sometimes known to reach 40 pounds in England, and in Scotland would probably amount to more." The greatest that might be exerted over the whole extent of such a surface as the Tay bridge was 10 pounds.

Mr. Gilke, previous to the accident, read a paper in which he took the total surface exposed at about two-thirds of the amount assumed by London *Engineering*, and the pressure needed to overturn a carriage at $39\frac{2}{10}$ pounds.

Prof. Stokes, of Cambridge University, at the inquest, said he thought 10 pounds per square foot, on a plane surface, as too low. The more rapid the *fluctuations* in the wind, the less their extent laterally. As far as experiment went the pressure was found to be nearly in proportion to the area of surface. He should like to see an exhaustive series of experi-

Velocity of wind.		Pressure in pounds per square feet normal plane surface=P.						Names given to winds.		Classification by Smithsonian Institution.
				Hagen, Eq. (2).						
Feet per second.	Miles per hour=V.	Rouse, Eq. (1).	Hutton.	Circular surface.	Square surface.	Triangular surface.	Smithsonian Institution.	Smeaton—Rouse.	Smithsonian Institution.	
0.	0	0.	.....	.....	.....	.....	0.	.....	Calm.....	0
1.47	1	0.005	0.004	0.003	0.003	0.004	.....	Hardly perceptible.....	.....	.....
2.93	2	0.02	0.016	0.014	0.014	0.014	0.02	.....	Very light breeze.....	1
4.4	3	0.044	0.036	0.03	0.031	0.032	.....	Just perceptible.	.....	.....
5.87	4	0.079	0.065	0.054	0.055	0.057	0.08	.....	Gentle breeze.....	2
7.33	5	0.123	0.101	0.085	0.086	0.088	.....	Gentle, pleasant, wind.	.....	.....
14.67	10	0.492	0.405	0.339	0.345	0.353	.....	.....	.....	.....
17.6	12	0.711	0.583	.....	.....	.....	0.75	Pleasant, brisk, gale.....	Fresh wind.....	3
22.	15	1.107	0.911	0.763	0.777	0.795	.....	.....	.....	.....
29.34	20	1.968	1.62	1.356	1.382	1.413	.....	Very brisk.	.....	.....
36.67	25	3.075	2.531	2.119	2.159	2.208	3.	....	Strong wind.....	4
44.01	30	4.429	3.645	3.052	3.109	3.18	.....	High winds.	.....	.....
51.34	35	6.027	4.961	4.154	4.232	4.328	6.	.....	High wind.....	5
58.68	40	7.873	6.48	5.425	5.527	5.653	.....	Very high.	.....	.....
66.01	45	9.963	8.201	6.866	6.996	7.155	10.	.....	Gale.....	6
73.35	50	12.3	10.125	8.476	8.637	8.833	.....	A storm or tempest.....	.....	.....
88.02	60	17.715	14.58	12.208	12.437	12.719	18.	A great storm.....	Strong gale.....	7
110.	75	27.65	23.781	.....	.....	.....	.....	.....	Violent gale.....	8
117.36	80	31.49	25.92	21.701	22.11	22.613	.....	A hurricane.....	.....	.....
132.03	90	39.852	32.805	.....	.....	.....	.....	.....	Hurricane.....	9
146.70	100	49.2	40.5	33.908	34.546	35.332	.....	A hurricane that tears up trees & carries buildings before it.	Most violent hurricane.	10

ments made on large surfaces. There were difficulties in recording velocities of the wind for short spaces of time. There is a difficulty in connecting the velocities with the pressures. Hydrostatically measured, a pressure of one pound on the square foot gave a velocity of 20 miles per hour. Experiment had shown that about 20 per cent. must be added to what is called the standard pressure (that on the surface of water in a tube), in order to get the algebraic sum of pressures per foot on both sides of a plate.

Mr. Scott, Secretary of the Scottish Meteorological office, said: "The greatest velocity registered at Glasgow (during the storm which destroyed the bridge), for any continuous 60 minutes, was 71 miles; but for one five minutes interval, it was 96 miles, for one 3 minutes interval, 120 miles, and for three others, 110 miles per hour." The storm gust at the Tay bridge was probably 300 feet wide, and some gusts may be restricted to 30 or 40 feet width.

Passing to other known disasters from wind pressure, the destruction of the Arrah road bridge across the Sone canal in India, is worthy of mention, since the weights, friction, &c., have been accurately calculated (see London *Engineering*, June 25, 1880).

This was 99 feet long, 15 feet wide, and continuous over two spans of 45 feet each. It was destroyed by a very narrow belt of wind. There were no holding down bolts, and the structure was lifted and pushed bodily off the supports, with almost no injury to the masonry.

Taking the whole vertical surface of both trusses, and the wind horizontal, and assuming the friction at 50 per cent. of weight, the pressure on the supposition of the wind striking the uncompleted span first, would be $20\frac{8}{10}$ pounds. If moved bodily, it must have been $29\frac{6}{10}$ pounds. Calcutta was the nearest point where records were kept, and during the cyclone 50 pounds were registered.

Coming nearer home, Mr. Francis, in his report on the chimneys of the Lawrence Manufacturing Company (published in *Engineering News*, Aug. 28, 1880), states the highest recorded pressure for fifty years past on the New England coast to be 50 pounds.

Mr. C. Shaler Smith, in his paper before this society of December 15, 1880, states pressures in the Western States at from 18 to 93 pounds, but does not state how the results were arrived at.

Mr. W. Hartnell, in a discussion in *London Engineering*, of January 2, 1880, states, respecting the gale in 1868, during which a momentary pressure of 80 pounds was recorded at the Bidston Observatory in Liverpool, that on the side of the observatory farthest from the sea, the pressure was only 38 pounds. He says further, that behind a flat plate a *partial vacuum* is formed by wind rushing past the edges. One per cent. of vacuum would give an increase of pressure per foot of 21 pounds. He says that narrow, flat bars, and open structures generally, offer a *maximum* resistance per foot of area. So that a statement of so much pressure means nothing, unless we know just how it is taken. He also says that a squall in still air will travel, say 20 miles per hour, and will blow at 30 miles for a short time, thus "exerting twice the pressure that might have been expected." In other words, that the maximum pressure is twice that due to the average velocity of the wind.

It is not necessary to go farther to illustrate the conflict of opinion which surrounds every branch of this subject.

There can be no doubt that much of the uncertainty arises from the use of imperfect apparatus, the *inertia* of the parts being entirely overlooked. On this point the article "Anemometer," in the *Cyclopedia Britannica*, although written some years ago, is, in a measure, true now. It says: "If the currents of air were anything like uniform, it would be a comparatively simple matter to deduce the velocity from the pressure; but their variability is so great that the relations between them become unworkably complex. We know from the elementary principles of dynamics that the pressure at any instant will vary as the square of the velocity. Obviously, therefore, the relative variations of the pressure will be *twice* as great as those of the velocity; and the *latter* are too great, as we find them to encourage us to double them artificially. It must be remembered also, that from the *inertia* of the indicating apparatus, errors will in every case arise, and *these* also will be doubled if we take the pressure instead of the velocity indications. From all this it will appear that comparatively little importance is to be

attached to the earlier, and to *all* statistical anemometry."

As an example of the apparatus used for recording pressures, may be instanced that at the Central Park in New York, as described in the report for the year ending December 31, 1869. A storm occurred December 18, 1869, which is fully recorded in the same report. The apparatus consists of a hollow metal cylinder 2 feet high and 1 foot diameter, suspended with its axis vertical by a chain, and having a chain extending from its lower end, and connecting with a spring balance. As the wind blows the cylinder to one side, the spring is extended, and a pencil attached to the spring is moved up, and records its excursion on a sheet of paper traveled slowly past it by clock work. By testing the power required to extend the spring to various points, it would seem that a fair measure of the pressure might be obtained.

On examining the record of the storm mentioned, however, it is seen to be a series of quick strokes outward, and back again nearly to the zero line. So much is this the case, that the paper is shaded almost black near the zero line, becoming lighter as we pass from it, and with a few detached strokes of no appreciable duration at the extreme pressure. The *greatest* pressure recorded is 22 pounds, and that only twice, and at long intervals. Of 18 pounds pressure or over, there were four at intervals of 12½, 30 and 35 minutes respectively. At 15 pounds and over, about 10, with intervals of 2½ to 30 minutes. These are approximations, as it is difficult to measure from the lithograph.

It is evident that these records are vitiated by the *inertia* of the cylinder, just as when a pound weight is suddenly placed upon a spring balance, about 2 pounds will be momentarily indicated.

The instrument can in no case, however, be considered a scientific one, and there seems to be a similar difficulty with most of those employed.

Another source of error is well pointed out by Mr. Ashbel Welch, in his paper before the Society, of May 25, 1880.

If we take the fundamental formula $h=\frac{v^2}{2g}$ and calculate the pressures corresponding to air at the standing temperature and density, the results given will be very closely one-half only of those given by Rouse and Smeaton. We know, however, that the actual pressures from the wind are greater. Could each particle of air give up all its motion, and get out of the way of those to follow without affecting the latter, the result would be rigorously true. There is, however, a piling up or condensation at the front or incident surface and a lateral flow—such, that the stream to be considered has virtually a larger section or base than the surface acted upon. In addition, there is the partial vacuum in the rear, noted by Mr. Hartnell, but first pointed out by some of the earlier experimenters on the subject.

These results, as we have seen, are modified not only by the shape of the incident surface, but also very largely by the length of the body acted upon in the direction of the wind current.

To sum up from the data given, we find the following sources of error, if we consider the maximum pressure of the wind, upon the exposed surface of a structure, as required by the engineer in proportioning its parts:

1st. Imperfect anemometers, vitiating results by the inertia of the moving parts, thus tending to make results too large.

2d. The difficulty of translating velocities into pressure, tending to make results too *small*, since the velocity recorded is an average over a period of time, and not that at the instant of greatest pressure.

3d. The data on record are not exact. To serve the purposes of science every circumstance as to size of parts, character and shape of surface, size of openings, height above ground, and location, must be noted.

4th. Almost all the records are based upon the assumption of a horizontal wind, whereas all experience shows that this is incorrect, and that the largest possible projection is liable to be the one acted upon.

In reference to height above ground, and direction of wind, the following memoranda are to the point.

In *Engineering News*, of September 11, 1880, is an article referring to experiments with a specially devised anemometer, by Mr. S. Fraser, of the Scottish Meteorological Society. He says that

wind currents are much modified by the ground over which they pass. For example, in front of the Royal Observatory in Edinburgh, their deflection from the horizontal was about 45°. On the level ground it was not over 15°, except in one case near Edinburgh, when it suddenly assumed nearly a *vertical* position. In the case already mentioned of the Arrah road bridge, it would seem that there was a strong lifting, as well as a horizontal force, to have removed the bridge with so little damage to the masonry.

As to the effect of height there is in the monthly weather report of the U. S. Signal Service for September, 1880, the following quotation from a "Report on Results of Wind Observations, made with small cup and dial anemometers at different heights, by Thomas Stephenson, M. I. C. E., published in the Journal of the Scottish Meteorological Society:

"Although additional observations are much wanted at high levels, the results, so far as appears from the observations on winds varying from 2 miles an hour to 44 miles an hour, show:

1st. That spaces passed over in the same period of time by the wind increase with the height above the sea level, or above the surface of the ground.

2d. The curves traced out by those variations of velocity (from 15 feet to 50 feet above the surface of the ground, and possibly higher) coincide most nearly with parabolas having their vertices in a horizontal line 72 feet below the surface.

3d. Between 15 feet and the ground surface there is great disturbance of the currents, so that the symmetry of the curves is destroyed.

4th. The parameters of these parabolas increase directly in the ratio of the squares of the velocities of the different gales:—if x be taken as the velocity of the wind at H feet above the ground the parameter of the corresponding parabola is $\left(\frac{x^2}{H+72}\right)$ and as x varies, the parameter will vary as x^2, or as the square of the velocity of the gale.

5th. In order to render wind observations comparable, all anemometers should, if possible, be placed at a uniform height above the ground, and that standard height should not be lower than 20 feet above its surface, but, were it generally practicable, 50 feet, or a still greater height, would be better.

6th. When it is desired to find for *small heights* the velocity V at any point H feet above the ground, from the known velocity v at a height h feet above the ground (h being *above* 15 feet), the formula is $V=v\sqrt{\frac{H+72}{h+72}}$; when H is above 50 feet above ground, the V got from the formula is slightly in excess of the actual velocity. When it is wanted to ascertain the velocity for *great heights* above the sea level, the approximately correct formula, which is believed to be sufficiently correct for practical purposes, is $V=v\frac{H}{h}$.

7th. It would be well for meteorologists to adopt this reduction formula and to express all wind velocities as referred to the height of 50 feet above the ground. This formula in this case becomes

$$V=v\sqrt{\frac{122}{h\times 72}}."$$

It is no wonder after what has been given, that engineers disagree as to the pressure of wind even in a given case. We find one man taking the vertical projection of one truss, another of one and a half, another of two, and so on, and almost no reference made to any other portion of the structure or to any differnet projection.

Again the same pressure is taken for a bridge in a clear, open space, or for a gorge where, as Mr. Cooper (Vol. IX—394 of "Transactions") has pointed out—there may be developed "local currents of much greater velocity," than the average of the storm.

Again, the same pressure is ordinarily taken for a short bridge, as for a long one, and we have a common agreement, among observers that the violence of storms is exerted over only limited areas, and is progressive in its character. There is also the other fact, that for structures of great span, the danger from repeated or "rhythmic" gusts (spoken of by Mr. Cooper) is exceedingly small. An examination of the storm record, given in the Central Park report, will show that the

gusts are hardly such as to cause this danger to structures whose period of vibration is very slow.

The experiments and observations needed to secure a scientific treatment of the whole subject, and uniformity of practice among engineers, must be more exhaustive than any heretofore made; but we shall never reach the desired end by assuming that it is unreachable. In the hope that this review may assist in promoting a movement in the right direction, this paper is respectfully submitted.

THE PRESENT STATE OF THE WATER SUPPLY AND SEWAGE QUESTIONS.

Abstracts of Papers of the Chemical Society and the Sanitary Institute of Great Britain.

A PAPER on "River Water," by Dr. Meymott Tidy, before the Chemical Society, was mostly a reiteration of the principles advocated in a former communication, in reply to Dr. Frankland. The discussion relates to the agencies to which the self-purification of running water is due; whether subsidence, the scavenging propensities of fish, or the oxidation effected by the atmosphere and plant life jointly.

First, as to the oxidation of peat, notwithstanding Dr. Frankland's objections, the author still maintains that the Shannon is admirably adapted to prove oxidation, should such a process be going on, admit the ever recurring entrance of feeders of peaty water if it can be shown that in spite of this constant addition of peaty matter there is a manifest lessening of the peat in the water, then the experiment, considering that all the chances and conditions are against proving oxidation, becomes a hundredfold more conclusive than it would otherwise be. Admitting that at four points along a course of forty miles at Portumna, Killaloe, O'Briensbridge, and at the junction of the Mulcaire the organic elements do indicate a fairly uniform quantity of peat in the water, the question remains, what has become of the floods of peaty matter which have entered the river during its forty-mile flow? Why does not the peat in the river increase in quantity yard by yard? Starting with a brown water at Killaloe, the river should be black over and over again before it reached Limerick. The author sums up this part of his paper thus: "I cannot question for a moment that peat is got rid of in the course of the flow of a river, and that oxidation is one of the agencies concerned in its accomplishment." He then criticises the paper by Miss Halcrow and Dr. Frankland on the action of air on peaty water, and objects that in no sense are these experiments, in which the same dribble of water is brought ever and anon into contact with the same few bubbles of air, comparable to the case of the Shannon, a river of great volume, in free and open contact with an ever-changing sea of air, luxuriant with vegetable life and fish. In one series of experiments in which some peaty water was allowed to remain for a year in contact with some air in a stoppered bottle, the author objects that, admitting oxidation to have occurred at the layer where actual contact took place between the water and air, any carbonic acid formed would act as a party wall to divide the water from the purer air above, and, making all allowances for diffusion, there practically would be an end of the process. He also points out that the carbonic acid was in enormous excess in the air above the water, viz., 14.7 parts in 10,000. Dr. Frankland's experiments on the Irwell, the Mersey, and the Darwen are next considered. "These are," says the author, "notoriously polluted sewage rivers, containing no vegetation, devoid of fish, and everything was against the regaining of purity," yet Dr. Frankland's results show, in the case of the Darwen, a decrease of organic elements of 41 per cent., and in the Irwell of 31 per cent. Again, in the case of the Tees, a river polluted at Barnard Castle by the sewage and refuse from dyers and fellmongers, after a flow of sixteen miles this river is reported at Darlington, by Dr. Frankland, as of unimpeachable quality, bright and palata-

ble. As Dr. Frankland's statements, that in the Severn a flow of thirty yards reduces the organic elements by 32 per cent.. and that after a flow of a mile they were only reduced 12 per cent. more, the author contends that sewage is not of a constant composition, and that some constituents are much more easily oxidized than others. As regards the wonderful diminution of chlorine remarked by Dr. Frankland, the author still maintains that it was taken up by vegetation, and quotes an experiment in which he watered some watercress with a solution containing 40 grains of salt per gallon, and increased the quantity of chlorides by 220 per cent. He gave the statistics of ten years, which prove that zymotic diseases do not necessarily result—sewage or no sewage—from drinking river water, any more than they can be kept away by drinking chalk water. He asks Professor Huxley this question: Can you give one single well-authenticated case where a drinking water, in which the chemist failed to detect manifest contamination, has caused disease? The author concludes by asking Dr. Frankland, as the dangerous element in a water is entirely outside his ken or detection, what in his judgment is the good of water analysis? Seeing that 100,000 bacteria may be present in a gallon of water, each bacterium being capable of imparting disease, and yet be undetected by the most refined chemical processes, how can he report any water to be wholesome? What are his grounds for reporting a water containing 0.1 of organic carbon to be of good quality, and a water containing 0.4 of organic carbon to be of inferior quality, seeing that the first named contained millions of bacteria, which may be entirely absent from the second? Why should absolute immunity from epidemics of cholera be promised, if only London would drink chalk water—which after all is merely rain water, and must have fallen in certain parts on heavily manured land, and has afterwards filtered naturally through chalk—and yet it be stated that there is not a tittle of trustworthy evidence to prove that artificial filtration—which, judging from analyses, is more complete than natural filtration—affords any safeguard against the propagation of epidemic diseases?

Dr. Frankland said that it would be difficult to notice at that late hour all the salient points of Professor Tidy's elaborate paper, but he would endeavor to refer to some of them. Professor Tidy has explained that the purification of rivers is effected by three processes—subsidence, the action of fish life, and oxidation. As regards the second, he does not clearly state whether the fish remove organic matters in suspension or in solution. Fishes of respiration must clearly remove oxygen from the water; so it would seem that they could only remove organic matter in suspension. However, the substantial difference of opinion was on the question of oxidation, to which he would therefore confine his remarks. He would have liked to have said something about the admirable paper by Mr. Hatton, but as time pressed, he must leave the results to speak for themselves. First, as to the oxidation of peat in the Shannon, Professor Tidy states that in a flow of one mile from Killaloe the organic carbon decreases from .8 to .48. This really means that half of the total organic matter has been converted into carbonic anhydride, water, and ntirogen, or ammonia, in the flow of one mile. He would ask any chemist who has been accustomed to organic compounds whether he is acquainted with any substance which behaves in this way. To take one instance—aldehyde—a substance most prone to decomposition, it, under similar circumstances, would hardly be oxidized perceptibly. How improbable is it that the flow of a mile should cause such an enormous reduction, whilst the exposure of fifty-one square miles of surface in the loch above should have such a slight influence! Does Professor Tidy really think that this entrance of feeders of peaty water has any very great influence on the composition of the Shannon, a river which drains about a quarter of Ireland? None of these feeders have a flow of more than a mile, and so they cannot be insignificant. He must take exception to two of the three new analyses which Professor Tidy gives, namely, those taken one mile below Castle Connell and at Limerick. Now, the entrance of the largest tributary—the Mulcaire—a river containing but little organic carbon and much suspended mineral matter, which,

in its deposition, carries with it much organic matter—completely prevents any fair comparison between these two places. Notwithstanding Prof. Tidy's great faith in nature, and his objection to any experiments in a bottle, the speaker ventured to say that in the case of the bottle, which was fastened to the connecting rod of the steam engine, the water was brought into contact with air much more perfectly than in nature, and yet no oxidation could be detected.

The discussion of the sewage question, before the Sanitary Institute, has been revived by an address by Prof. W. H. Corfield, of which the following is a brief report:

That the removal of refuse matters was a highly important matter for consideration was very evident, the lecturer thought, from the fact that wherever a district had a slow system of removal or none at all, the death rate was very high, and just in proportion as the removal was expedited the number of deaths and cases of enteric disease decreased. The results of the non-removal of refuse matters was shown by the spread of black death, Oriental plague, cholera and enteric fever. The entire depopulation of many ancient cities was, he believed, due to the injurious effects of the accumulation of filth. Having pointed out that utilization, though no doubt important, was a secondary matter, and indeed was what an athlete would call "a very bad second," the reader of the paper proceeded to consider the systems of sewage removal and treatment at present in use under the two heads of conservancy systems and water carriage. In the former—which might have been so named by their bitterest opponents, instead of the title being one adopted by their advocates, as the very object of the sanitarian was "removal," not "conservation"—the refuse matters were either collected unmixed with anything else, or mixed with ashes, earth, &c. The first of these plans was now being adopted in several large towns, as Birmingham, Rochdale, &c., and it might be said to be the only successful one among the conservancy systems. It was certainly the only one from which a profit had been obtained; the manure in all the other plans was nearly valueless, and the results obtained by the Sewage Committee of the British Association showed that the earth compost, after having been used in the closets six times, was only as rich as a good garden soil, and would not bear the cost of carriage. The total manurial value of human excreta had been estimated at 7s. 3d. per year for the liquid refuse, and 1s. 3d. per year for solids, but all chemical analyses of manures placed their value at a higher rate by about one-half than they possessed to a farmer. A summary was given of the results obtained by the conservancy systems, in which they were shown not to be solutions of the questions, especially as they leave the liquid sewage still to be treated, but in which it was admitted that under certain circumstances as where it was necessary to reduce the bulk of the sewage, they might with proper precautions be adopted. The recent improvements had been in the direction of reducing the size of the receptacles, so that they had to be removed at shorter intervals, in making them water tight and in ventilating them. The interest of a local authority, and still more of its contractors, was to empty the pails as seldom as possible—a radically bad principle. The water-carriage system was considered somewhat in detail; its advantages in the continuous removal of refuse from houses pointed out, its disadvantages shown not to be inherent in the system, but to the mistakes made in carrying it out, as for instance, sewers pervious to water, or too large, or not ventilated, or without sufficient fall, or with a blocked outlet, or discharging into rivers, house draining not properly disconnected. The folly of turning surface water, and, in many instances, even springs and streams into sewers, and so increasing the difficulty of dealing with the sewage at the outfall was insisted on. The various chemical processes for the treatment of sewage were passed in review, and all shown to be quite inadequate to cope with the difficulty, though some might be useful as preliminary aids to purification. Filtration through soil and wide irrigation were next treated at some length, and the results obtained by them described. Certainly the sewage had been satisfactorily purified in many cases, the conditions

for satisfactory purification in winter being that the sewage pass through the soil and not merely over it. Crops of all kinds had been grown by means of it, and in soil that would otherwise bear nothing; and the British Association's Sewage Committee had shown at Britton's farm, Romford, during five years' daily investigation and analysis, that as great a percentage of the manurial constituents, 33 per cent., of nitrogenous compounds had been utilized as was on an average utilized of the best commercial manure. Although, for various reasons, it had seldom been found to be remunerative, the reader adhered to his opinion, formulated ten years ago, that sewage irrigation would ultimately be remunerative in many instances, and that opinion was shared by the committee appointed by the Local Government Board in 1876 to inquire into modes of treating town sewage. An irrigation farm should be supplemented by a filter bed to receive and purify sewage when it is not wanted on the farm. The supposed dangers from the proximity of such farms, or from the spread of entozoic disease, were found to be purely imaginary; it was on the whole a better solution of the question for a large number of places than any other, and if, as was very likely, we had a series of dry years, its adoption would receive a great impulse. Where towns could not make sewage utilization pay, they must be content to be taxed to a slight extent to get rid of a most serious nuisance, and to secure a low death rate.

A short discussion followed, in which Messrs. Sillar advocated the Aylesbury precipitation process, in which he was he said, pecuniarily interested; Mr. Douglas Onslow, managing director of the Coventry sewage purification works, defended the operations there carried out; Mr. Bailey Denton, Jr., spoke of the intermittent downward filtration works carried out for many years past by his father; Mr. R. W. P. Birch agreed with the lecturer, that no fine line should be drawn between irrigation and intermittent filtration, but that on the sewage farm both plans should be adopted as the season demanded; Mr. Jeumain described Lord Warwick's sewage farm at Leamington, and asserted that sludge had a manurial value; and Mr. Thomas Wilson Grindle suggested that as Hillé's system, which he was carrying out at Edmonton, was practically the same as the Aylesbury and Coventry processes, a conference should be convened to arrive at an amalgamation of the several sewage companies.

The Chairman said this was attempted at the Leamington congress 16 years since without any result. A long experience in the working of the Croydon sewage farm, which was situated in a somewhat densely populated area, had shown him that this was the best means of sewage disposal, and that it was absolutely no nuisance or danger to the neighborhood. Forty tons of vegetables could exhaust the manurial qualities imparted to land by irrigation with 5,000 tons of sewage, so that its fertilizing properties had been overrated. The practical difficulty was to get an intelligent farmer to occupy such a farm under so changing and occasionally so arbitrary and ill-informed a body as a "local authority."

Dr. Corfield replied upon the discussion, ridiculing the notion that any plan of "fortifying" sewage could pay. Doubtless sand could be made to pay for carriage and treatment if gold dust were sprinkled over it; but no one would assert that the enrichment with gold dust, to be subsequently extracted was a remunerative or wise enterprise. The simile held good with all additions to sewage; the stuff itself was worth, on a favorable computation, about 2d. per ton, and the cheapest chemicals that could be added to it would not make the compound worth carriage, plus their original cost to the farmer. A vote of thanks was passed to the lecturer on the motion of Mr. Rogers Field and Professor Symons.

THE Municipal Council of Paris has recently made a great improvement in the arrangements of the Morgue, by adopting the refrigerating apparatus of M. Mignon and Rouart at a cost of 53,000f. The bodies on view will thus be enabled to be preserved for any length of time within reason, and the sanitary conditions of the Morgue will be greatly altered for the better, while the longer period of exposure will frequently further the ends of justice and give more frequent opportunities for identification.

ON CERTAIN REACTIONS IN THE BASIC CONVERTERS.*

By M. POURCEL.

From "Iron."

THE experiments, the result of which I gave at the last meetings of the society, prove that the phosphate of lime when tribasic, and even when it contains more than three equivalents of lime, is, in the absence of every trace of silica, decomposed partially by the manganiferous pig (or otherwise) which absorbs the phosphorus. I had then, and, I admit, erroneously, expressed an opinion contrary to that of Mr. Rollet. Mr. Rollet's opinion is, that a silico-phosphate of an alkaline earth, sufficiently basic, does not yield any of its phosphorus to pig iron melted in contact with it; but if the silico-phosphate contains phosphate of iron in admixture, the latter is reduced by the pig, which completely absorbs the phosphorus.

The following experiments tend to verify these two facts:

(1) A kilogramme of white pig iron containing .06 per cent. of phosphorus was melted in a plumbago crucible, lined with a brasque of calcined dolomite, with 225 grammes of slag specially prepared of the following composition:

	Per cent.
SiO_2	15.00
Ph_2O_5	9.40
CaO	58.75
MgO	10.62
$Al2O_3$ and a little Fe and Mn	5.50
S	0.73
	100.00

The fusion occupied two hours. The button of iron run into a mould weighed 997 grammes; it contained 0.101 per cent. phosphorus. The accompanying slag contained 14.30 per cent. silica, and 10.44 per cent. phosphoric acid. Consequently the pig, notwithstanding its prolonged contact with the silico-phosphate, had absorbed scarcely any phosphorus. If, as is the case in making the final additions in the basic converter, the contact with the slag had been of short duration, the absorption of phosphorus would have been "nil."

(2) The same experiment was repeated in a plumbago crucible, brasqued with vegetable charcoal as free from ash as possible. The slag had been made so as to contain the same quantity of phosphoric acid, but about 20 per cent. of silica. The fusion occupied the same time as in the preceding experiment. The button of iron could not be weighed. Its fracture was that of a fine-grained grey pig. The phosphorus was 0.105 per cent., and the accompanying slag contained 21 per cent. of silica. Consequently, even with a slag containing 21 per cent. of silica, and 10 to 11 per cent. of phosphoric acid, the absorption of phosphorus is inappreciable.

(3) In a plumbago crucible, brasqued with vegetable charcoal, a kilogramme of the same white pig iron containing 0.06 per cent. of phosphorus was melted with a mixture of 112 kilogrammes of the slag used in the first experiment, and 41 grammes of phosphate of iron, containing in round numbers 15.5 grammes of phosphoric acid, and 25.5 grammes of oxide of iron. Reduced to percentages this slag was composed as follows:

	Per cent.
SiO_2	10.90
Ph_2O_5	17.30†
CaO	43.00
MgO	7.80
Al^2O_3, &c	4.00
Fe_2O_3	17.00
	100.00

Fusion was completed in about two hours. The button of metal run into a mould weighed 1024 grammes; it presented a close-grained dark fracture. The percentage of phosphorus was 0.64 per cent. If all the phosphate of iron had been reduced to phosphide and incorporated with the pig, supposing there were no loss of iron whatever, the theoretical content would have been 0.68 per cent. This result at first surprised me, and I can only explain it from the manner in which the experiment was con-

* A communication addressed to the Société de la Industrie Minérale.

† 7.15 per cent. combined with the silico-phosphate. 10.15 per cent. combined with the oxide of iron.

ducted. The phosphate of iron in a powdered state was placed in the bottom of the crucible in contact with the reducing brasque, and covered by the silico-phosphate in lumps—possibly the reduction of the phosphate to phosphide was effected before the complete fusion of the silico-phosphate; this may explain its almost total incorporation with the pig.

Generally when a silico-phosphate is not saturated, the phosphate of iron melted in contact with it is partially decomposed, oxide of iron is set free, and the phosphoric acid passes into the silico-phosphate. Such is the reaction that takes place in the basic converter during the overblow in the presence of an excess of lime. When, however, the lime is not in excess, the slag may contain the greater part of the phosphoric acid as phosphate of iron, and the re-integration of the phosphorus in the bath of refined metal becomes more apparent when the spiegel is added.

Such was the case at the first experiments of the Thomas-Gilchrist process at Eston.

I myself came to the conclusion after making some laboratory experiments—which, however, were not sufficiently conclusive—on slag produced at the Eston works, that the whole of the phosphorus in the slag existed as a phosphate of iron; but this I acknowledge was an error. The experiments which I have just described may, I think, be summed up as follows:

(1) That a slag containing from 15 to 20 per cent. of silicon, and 10 per cent. of phosphoric acid, will not part with an appreciable quantity of its phosphorus on the addition of the spiegel.

(2) That this re-integration of the phosphorus is proportional to the quantity of phosphate of iron dissolved in the slag, a silico-phosphate of fixed and definite composition; and then proportional to the weight of the spiegel added at the end of the blow. These views are, I believe, not at variance with those of Mr. Rollet. The hypothesis of Dr. Wedding, admitted by many German professors, that the manganese of the spiegeleisen is the reducing agent of the phosphoric acid, incorporated in the slag as phosphate of lime, does not then appear to be justified. We are not here dealing with a phosphate of lime; but, as I observed in my first communication on the Thomas-Gilchrist process in June 1879, with a silico-phosphate with an excess of base, which does not give up its phosphorus.

It may be asked whether phosphate of lime dissolved in fluor spar would not be altered by fusion in contact with pig iron. My belief is that it would be, and an experiment in confirmation of this would not be without interest. I had ample proof from an experiment made in September, 1879, that it was possible to eliminate simultaneously carbon and phosphorus from pig iron, by means of fluor spar and an oxidizing agent. The experiment I then made was as follows: 180 grammes of a fusible basic compound formed of 100 grammes of pure lime and 80 grammes of fluor spar were melted in a plumbago crucible, with 100 grammes of a pig, containing 17 per cent. of phosphorus and about 1.25 per cent. of carbon estimated by difference, and 200 grammes of oxide of iron ($Fe2O_3$) precipitated, containing 62 per cent. of iron, and 3.40 per cent. of water. This mixture was calculated to furnish sufficient oxygen to burn the carbon, and oxidize the phosphorus of the pig, in order to form a very basic phosphate of lime. A button of metal was obtained, weighing 196 grammes. The fracture was that of a crystalline soft steel. It forged very easily, and contained

Iron	99.146
Carbon	.467
Phosphorus	.380
	99.993

There was a considerable quantity of slag, which contained some shots of white metal. It was white, and yielded on analysis, 1.90 per cent. of iron and 3.463 of phosphorus; the weight of the well melted, but somewhat honey-combed part, was 461 grammes. This considerable weight is attributable to the melting of the dolomite brasque of the crucible.

In the numerous experiments that I have made in crucibles of this class, the graphitic lining alone is damaged, whilst the dolomite part remains hard, free from cracks, and, placed in a new graphite crucible, may serve for a great many experiments. But, in the presence of fluor

spar, the dolomite brasque is always strongly attacked, and if, instead of melting together the mixture of lime and fluor spar in a carbon medium, the fluor spar be put separately in the crucible to be used in the experiment, the crucible is generally eaten through before complete fusion has been effected of the various substances mixed together. In the experiment just described, even if an exact debtor and creditor account of the materials charged, and the product, cannot be given, still the balance sufficiently approximates to the truth, to prove that the phosphorus of the pig iron attracted by its affinity for *lime in fusion*, acted in preference to the carbon as the reducer of the oxide of iron. The latter was almost completely reduced, since the slag contained but 1.90 per cent. in the state of oxide, or rather less than 9 grammes in all. As regards the carbon of the phosphoric pig—and this is the principal fact in the experiment—it was found to remain in a great measure in the metal. Of the 1.25 grammes contained in the pig charged, we have in the product $196 \times .467 = .915$ grammes.

I communicated the result of this experiment to some of my friends at the time, but I had reasons for not publishing the details. Referring to the *Compte Rendu* of the meeting of October 4, 1879, page 227, I there, in answer to Mr. Laur, contented myself by replying in the following terms: "The chlorides of iron are volatile, in fact, extremely so. My conviction is, contrary to that of Mr. Laur, that apatite is not likely to be formed; under the action of the iron, chloride of calcium will yield thick fumes of volatilized chloride of iron, with fluoride of calcium. There will also be a loss of iron in the state of fluoride; but the principal reaction to be feared is the eating away of the sides and bottom of the vessel."

I could not deny the action of fluor spar, of which I was already aware; I merely mentioned the trouble to which its use was likely to give rise. If the experiments which Mr. Harmet related as having been carried out at Bochum, have not been repeated since last November, it is probably due to difficulties existing beyond the cooling of the bath of metal from the heat absorbed by the dissolution of the lime in the fluor spar, and the formation of volatile compounds. The desulphurizing influence of fluor spar, or of lime dissolved in borax, in a reducing medium, was the subject of a note I presented to the Société de l'Industrie Minérale, August 5th, 1876. The conclusion at which I then arrived, was that lime, under whatever form it may be introduced into a slag, retains the sulphur as a sulphide of calcium, when the temperature is high enough to permit the fusion of a basic slag.

OBITUARY.—Died, on the 14th of July, at Rio Janeiro, Col. W. Milnor Roberts, past President of the American Society of Engineers, and late Chief Engineer of the Public Works of Brazil.

From a history of his professional career, recently published in the columns of the *Engineering News*, we take the following sketch:

"Col. Roberts was born in Philadelphia, Feb. 12, 1810. His aptitude for mathematics early introduced him to the then new profession of civil engineering, and in the spring of 1825 he received his first appointment as a chainman of the Union Canal, of which Canvass White was Chief Engineer, and Sylvester Welch, Locating Engineer. At 18 years of age he was appointed engineer in charge of the most difficult division of the Lehigh Canal, from Mauch Chunk down, sixteen miles, and from that time forward he was always intimately connected with great canal and railway enterprises, principally in Pennsylvania and New York States, with intervals in Brazil, and in the Western States. He held important offices under the United States Government, was Chief Engineer of the Northern Pacific Railway, Associate Chief Engineer of the St. Louis Bridge, and an active and important member of the Mississippi Jetty Commission. In 1879, shortly previous to his departure for Brazil, Col. Roberts was elected President of the American Society of Civil Engineers, a society of which he was a very active and always interested member, and which will very keenly feel his loss. Though so far advanced in years, Col. Roberts was an unusually active and energetic man, and some idea of the extent and difficulty of his labors in Brazil may be gathered from letters which have been

published in this journal during the past two years. Col. Roberts was possessed of a most genial and kindly disposition, and the news of his death will be received with feelings of great sorrow by the entire profession of which he was a member, as well as by a very large list of friends in this and other countries where he was known.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS. The last No. of the Transactions contains the following papers:

No. 220. "Wind Pressure upon Bridges," by C Shaler Smith.

No. 221. "An Examination into the Method of Determining Wind Pressure," by F. Collingwood.

The discussions upon these two papers, which also appear in the present issue, express the views of several prominent members, and contribute to make this an exceedingly valuable number.

CLEVELAND ENGINEERS' CLUB.—The July meeting of the Engineers' Club was held on the evening of the 6th, with Colonel J. M. Wilson in the chair and a large number of members in attendance. The following papers were presented:

Paper—"Remarks on George Stephenson," by A. Mordecai.

"Buildings in the City, Public and Private," by J. M. Richardson.

"Connotton Valley Railroad—Its Progress, Lines and Bridges in the City," by H. F. Dunham.

"Results of the Annual Meeting of the American Society of Civil Engineers at Montreal, in June, 1881," by Charles Latimer.

"Candle Power of light—How Determined," with experiments, by G. A. Hyde.

"Recent Break in Case Avenue Main Sewer—Cause, Heavy Land Slide," by B. F. Morse.

No meeting held in the month of August.

THE INSTITUTION OF CIVIL ENGINEERS.—The originality, labor and ingenuity displayed by the authors of some of the communications submitted to this Society during the past Session, have led the Council to make the following awards:

FOR PAPERS READ AT THE ORDINARY MEETINGS.

1. George Stephenson Medals, and Telford Premiums, to Thomas Forster Brown and George Frederick Adams, MM. Inst. C.E. for their paper on "Deep Winning of Coal in South Wales."

2. A Watt Medal and a Telford Premium, to John Isaac Thornycroft, M. Inst. C.E., for his paper on "Torpedo Boats and Light Yachts for High Speed Steam Navigation."

3. A Telford Medal and a Telford Premium to Theophilus Seyrig, M. Inst. C.E., for his paper on "Different Modes of Erecting Iron Bridges."

4. A Telford Medal and a Telford Premium to Max am Ende, Asso. M. Inst. C.E., for his paper on "The Weight and Limiting Dimensions of Girder Bridges."

5. A George Stephenson Medal and a Telford Premium to Benjamin Baker,* M Inst. C. E., for his paper on "The Actual Lateral Pressure of Earthwork."

6. A Telford Premium to Richard Henry Brunton,† M. Inst C.E., for his paper on the "Production of Paraffin and Paraffin Oils."

7. "A Telford Premium to Charles Colson† Asso. M. Inst. C.E., for his paper on "Portsmouth Dockyard Extension Works."

8. A Telford Premium to Christian Hendrick Meyer, Assoc. M. Inst. C. E., for his paper on the "Temporary Works and Plant at the Portsmouth Dockyard Extension."

9. A Telford Premium to Benjamin Walker, M. Inst. C.E., for his paper on "Machinery for Steel Making by the Bessemer and the Siemens' Processes."

10. The Manly Premium to Joseph Prime Maxwell, Assoc. M. Inst. C E., for his paper on "New Zealand Government Railways."

FOR PAPERS PRINTED IN THE PROCEEDINGS WITHOUT BEING DISCUSSED.

1. A Telford Medal and a Telford Premium to Professor Dr. J. Weyrauch, for his paper on "The Calculation of Dimensions as Depending on the Ultimate Working Strength of Materials."

2. A Telford Premium to James Richard Bell, M. Inst C.E., for his paper on "The Empress Bridge over the Sutlej."

3. A Telford Premium to John Lewis Felix Target,† M. Inst. C.E., for his paper, Experiments on Modules for Irrigation Purposes."

4. A Telford Premium to William Thomas Henney Carrington, Assoc M. Inst. C.E., for his paper on "Three Systems of Wire Rope Transport."

FOR PAPERS READ AT THE SUPPLEMENTAL MEETINGS OF STUDENTS.

1. A Miller Prize to James Bernard Hunter, Stud. Inst. C.E., for his paper on "Wood-Working Machinery as Applied to the Manufacture of Railway Carriages and Wagons."

2. A Miller Prize to Mathew Buchan Jamieson, Stud. Inst C E., for his paper on "The Internal Corrosion of Cast-Iron Pipes."

3. A Miller Prize to Thomas Stewart, Stud. Inst. C.E., for his paper on "The Prevention of Waste of Water."

4. A Miller Prize to William Henry Edinger, Stud. Inst. C.E., for his paper on "Brick and Concrete and Concrete Gas-Holder Tanks."

5. A Miller Prize to Daniel Macalister, Stud. Inst. C.E , for his paper on "Caissons for Dock Entrances."

6. A Miller Prize to Lindsay Burnet,† Stud. Inst. C.E., for his "Description of a Cargo-Carrying Coasting Steam Ship, with Detailed Investigation as to its Efficiency."

7. A Miller Prize to Edward Walter Nealer Wood, Stud. Inst. C.E., for his paper on "The Improvement of the Old Harbor at Holyhead."

8. A Miller Prize to Arthur Stuart Vowell, Stud. Inst. C.E., for his paper on "Steel; its

Chemical Constitution and Behavior under Tensile Strain."

9. A Miller Prize to William Marriott, Stud. Inst. C.E., for his paper on "Boilers."

ENGINEERING NOTES.

ASPHALT CARRIAGEWAY PAVEMENT.—At a recent meeting of the Streets Committee of the Commissioners of Sewers for the City of London, the following tenders were received for paving the roadway of Queen Victoria Street, from Mansion House to Cannon Street, area about 5,500 square yards:

Name of Contractors.	First cost per sq. yd.	Maintenance for 2 years free of cost to commissioners, and for 15 years (after 2 years expired), measured over the entire surface, at per sq. yd. per annum.	Total cost of pavement to commissioners at expiration of 17 years at per sq. yd.
French Asphalte Co., 27 Cornhill, E. C.....	13s. 6d.	1s.	28s. 6d.
Limmer Co., 85 Gracechurch St. E. C.....	13s. 6d.	8d.	23s. 6d.
Val de Travers Co., Old Broad Street, E. C.....	13s. 10d.	6d.	21s. 4d.

The Committee recommended the acceptance of the lowest tender; and it should be further mentioned that at the expiration of seventeen years the pavement is to be left in as good a state and condition as when first laid down, and with a smooth and even surface.

THE PITTSBURGH SUSPENSION BRIDGE.—The suspension bridge connecting Pittsburgh and Allegheny, which was partially destroyed by fire recently, was a structure of some importance in the history of bridges. The suspension bridge, which replaced an old covered wooden structure, was finished in 1860, and was considered the finest in the world. The builder, Roebling, had not then acquired the European celebrity which came from the erection of the Cincinnati and Covington bridge. Mr. Roebling had a *carte blanche* to do as he saw fit, though he submitted all his projects to the company as he went along, and they were all endorsed. So perfect was the work considered that when the Prince of Wales and suite were here they were astonished to find such a work of art in what was then a rather small provincial town. The Duke of Newcastle, who was a very practical man, scrutinized the bridge closely, and informed the Prince that it was the best bridge he had ever seen. Before the war a dollar went a long way; what cost $300,000 then would cost $500,000 now. Roebling, the builder, when asked what was the strength of the bridge, said he had ascertained the power of the greatest hurricane that ever passed over Pittsburgh prior to 1858, and multiplied this by nine, which represented the strength of the bridge. President Harper says such power gave the company sufficient confidence in the power of the structure to resist pressure that no insurance against flood damage was considered necessary, especially as the bridge had been put 5 feet above the flood mark of 1832. It was considered almost impossible to set the bridge on fire, so since 1859 it has carried no insurance of any kind, and the company not only must stand the loss of several weeks' wagon travel, but must repair the damage themselves out of their accumulations.

THE CHANNEL TUNNEL.—At a meeting of the proprietors of the South-Eastern Railway, held on the 16th inst., Sir Edward Watkin, M. P., the chairman, described the experimental works which had been undertaken, with the view to show whether the making of a tunnel under the Channel between England and France was feasible and practicable. The whole question, he said, divided itself into two parts. One was whether they could pass under the Channel through a stratum which was impervious to water. The second point was whether, by the aid of machinery, they could shorten very considerably the probable time of construction. What they had done was this: They had sunk two shafts on this side of the Channel; one at the Abbott's Cliff Tunnel, and the other on this side of the Shakespere Cliff Tunnel From the first of these shafts they had driven a gallery of from 800 to 900 yards, of a diameter of seven feet, which had all been excavated by machinery. Last week, with that machinery, which was not perfect, they excavated 67 yards of lineal distance on the extension of that gallery. If that were the maximum speed each week, it meant about two miles of progress a year. Of course, as they worked from two ends, and as the distance was only twenty miles, practically speaking, it meant five years to complete a gallery seven feet in diameter, as an experiment, under the whole length of the Channel. As to the second shaft, at the Shakespere Cliff, they had sunk that down to a depth of 155 feet. They had also bored from the bottom of the shaft to a further depth of 106 feet. They had found no trace whatever of water in the old grey chalk. There was a small quantity of water near the surface, but this was always expected. He therefore thought that solved the great questions of the speed at which they could go, and of the impermeability of the strata to leakages of water. On the other side of the Channel the French company had sunk two very important shafts, and they had found exactly the same results as had been ascertained on this side. As to the machinery, they were on the eve of concluding another arrangement with Captain English, Colonel Beaumont, and M. Pigou, the proprietors of the machine with which they had been working. Under this new arrangement, they would pay merely for the use of the machine, and by means of it they would carry those experiments considerably further. It had been arranged between the French and English committees that they should drive

through a heading of a further length of one mile on each side When these two miles were finished—and they certainly ought to be in six months—one-tenth of the question was dealt with. If that were successful he should, he thought, propose a further treaty with the French gentlemen under which the remaining nine miles on each side would be done, and they would meet in the middle of the Channel. If that were successful, the whole question was practically settled. Until the matter was proved, however, neither the French nor the British investor would be asked to embark capital in the undertaking. The South-Eastern shareholders were, as it were, the founders of the feast. They had taken all the risk, and they had authorized an expenditure of not more than £20,000 upon the affair. Now a great deal of that which they wanted to prove had been proved. He meant to ask them to consider how best to make what had been proved more positive, and then to consider whether they should not get up a small limited liability company, or other company, to take the matter in hand, without further interfering with the finances of the South-Eastern Company. This was a question deserving serious consideration at their hands. They must, however, never forget that it was absolutely essential that this tunnel matter should remain under South-Eastern control.

ORDNANCE AND NAVAL.

TWO NEW CLYDE STEAMERS.—Within a short time there have been hand over to their owners two splendid new Clyde-built steamers of totally different types, one of them having been both built and engined by Messrs. John Elder & Co., Glasgow, and the other built by Messrs. John Reid & Co., Port-Glasgow, and supplied with engines by Messrs. Rankin and Blackmore, Greenock. As they are both unusually fine examples of their respective types, it will not be out of place to give a short descriptive notice of them.

The vessel first referred to is a magnificent iron screw steamship named the Elbe, of nearly 5000 tons gross register, and is owned by the North German Lloyd Company, of Bremen. She is of the following dimensions: Length, 440 feet; breadth, 45 feet; depth, 36 feet 6 inches, and she is classed in highest grade of the Bureau Veritas, with several extras over their requirements, such as lower and orlop decks, and additional water-tight bulkheads. All the decks and deck work are constructed either of teak or iron. In order to protect the vessel from the heavy Atlantic seas, strongly constructed iron turtle backs are placed over both ends of the ship. In addition to the accommodation for the officers and crew, 170 in number, the vessel is designed to carry 190 first-class, 120 second-class, and 1000 steerage passengers. The chief saloon, as is generally the case now-a-days in first-class vessels, is placed forward of the engines and boilers. It is a very handsome, beautifully lighted, and comfortable apartment, about 40 feet square, and was designed by Mr. Poppa, architect, Bremen, the style adopted being the German Renaissance. The Elbe is rigged with four pole masts of iron, with yards on the fore and main masts; and she is provided with steam windlass, steam and hand-steering gear, steam winches, steam hold pumps, steam "navy" pumps, fresh-water condenser, &c.; indeed, there are combined in her all the modern appliances for securing the safety of the vessel at sea, and facilitating the working of the cargo. The engines of the Elbe are of the three-cylinder type—one high-pressure cylinder having a diameter of 60 inches, and two low pressure cylinders each of 85 inches diameter, and having a stroke of 5 feet. The boilers, four in number, are double-ended, each 15 feet in diameter by 17 feet 6 inches long, constructed of iron for a working pressure of 80 pounds per square inch. There are 24 furnaces, made of mild steel, on Fox's patent corrugated system. Among the improvements in the machinery of the Elbe, mention may be made of the crankshaft, which consists entirely of Krupp's crucible cast steel, and is built up of separate pieces on a system introduced by the builders. The propeller shaft is hollow and made of Whitworth compressed steel. In accordance with Messrs. Elder & Co.'s recent practice in fitting out high-speed steamships, the propeller blades of the Elbe are made of manganese bronze, a material which is rapidly superseding iron and steel for the purpose in question. When running the measured mile in terms of the contract on the 18th of June, she attained a mean speed of 16.57 knots per hour, being .57 knots over that stipulated. One of the runs was accomplished at the rate of 17.145 knots. Altogether the engines worked very smoothly, there being no heating of bearings or priming of boilers. The indicated horse power attained was 6115, being about 700 above the contract figure, with 66½ revolutions per minute, working with a steam pressure of 76 pounds and 28-inch vacuum. On the previous day, when the builders' trial took place, the consumption of coal was 1.82 pound per indicated horse power per hour.

The other vessel, a very handsome steamer named the Laja, was, as we have said, built by Messrs. John Reid & Co., Port-Glasgow, and engined by Messrs. Rankin and Blackmore, Greenock, her owners being the Compania Sud Americana de Vapores en el Pacifico. She left the Clyde on the 18th for Liverpool *en route* for Valparaiso, to take up her station between that port and Panama. In many respects this vessel possesses features which differ entirely from those of the ordinary cargo and passenger steamer, inasmuch as the requirements of the trade in which she is to be engaged necessitated a special design as to internal construction. Her dimensions are as follows: Length, over all, 350 feet; breadth, 40 feet 6 inches; depth of hold to "shade deck," 39 feet; and her carrying capacity is about 3000 tons. The Laja has a double bottom, which is divided into four compartments for water ballast; and she has four decks, consisting of lower, main, awning, and "shade" decks. In the lower hold and 'tween decks there is space

for stowing 2000 tons of cargo, the loading and discharging of which are carried on from six side hatches on the main deck—three on each side—at all of which are placed steam winches. This latter deck has been specially fitted up for steerage passengers and cattle, provision having been made for 100 emigrants in the fore part, and between 500 and 600 cattle in the after part. On the awning deck the grand saloon, ladies' boudoir, state rooms, lavatories, and officers' quarters have been placed and fitted up in a style which reflects great credit on the skill and taste of the designers and builders, and bespeaks the liberality of the owners. The state cabin, which is placed amidships, is a very fine and airy compartment, measuring 45 feet by 30 feet, and liberally lighted from the roof and sides. We need not detail the decorations and fittings, but will content ourselves by saying that they are of great excellence and appropriateness; nor need we dwell upon the other accommodation which is provided for at least 124 first-class passengers. The "shade" deck of the Laja is a magnificent promenade, extending the entire length and breadth of the steamer. On the after part of this deck a kind of bazaar is usually held while the steamer is on her proper station. Natives are allocated so many feet of space upon which to expose their wares, which embrace every requisite and luxury. At each port of call the residents come on board, and there buy, sell, or barter—the steamer's deck being, in fact, the emporium whence the necessaries of life are distributed through various districts. Distilled water is also sold at places where fresh water is scarce. This floating and traveling bazaar is held on the after part of the deck, and in no way do the traders interfere with the passengers, who exclusively enjoy the fore part of the steamer. This deck is fitted up with the electric light on the Brush system, and the same light is made available for use in the loading and discharging of cargo by night. The captain's state room, chart room, &c., have been built on the fore part of this deck; but with the exception of a long row of seats which are placed down part of the center, the deck is nearly free from other incumbrance. The Laja has been fitted up with a great variety of modern appliances, including Paul's patent windlass, and Harrison's steam steering gear; and she is provided with nine life boats. She is rigged as a three-masted schooner, and has the old-fashioned clipper bow. Her engines are of the ordinary compound type, of 280 horse power nominal, and working up to something like 2500 indicated. The cylinders are 45 in. and 80 in. in diameter, with 54-inch stroke. She has four boilers, and has a four-bladed propeller which is made of steel. When on trial she made a speed of 14½ knots, which was deemed highly satisfactory, being much over the contract speed. Her machinery worked with the greatest smoothness during the trial.

RAILWAY NOTES.

RUSSIAN RAILWAYS.—Russian railways do not, from a commercial point of view, appear to be in a very satisfactory condition. The total returns of all the Russian railways are, according to the official reports recently published, 14 per cent. less for 1880 than they were for the year 1879. This reduced income is very unevenly distributed, for while on some lines it amounted to only 3 per cent. in 1880 against 1879, some of the southern lines showed a decrease of 25 per cent., and in one exceptional case even as much as 30 per cent. The three railways, the Nicolai, the Warsaw, and the Moscow Nishni Novgorod line, belonging to the great Russian Railway Company, had in 1880 a total income of £5,800,000, that is, £1,030,000, or nearly 18 per cent. less than in 1879. The reason assigned for this large falling off in railway returns is that it is due partly to a considerable reduction in the export trade, especially of corn, and partly to the inevitable consequences of a war, such as the Turko-Russian war; while if we accept the views of Mr. Karl Huber, late traffic manager of the Kharkov-Nicolaiev Railway, expressed in a lecture delivered some time ago at St. Petersburg, this unsatisfactory result is also largely occasioned by considerable extra expenditures, enforced upon the railway companies by some of the clauses of the Ministry for Communication, expenditures which, as the lecturer asserted, were in many cases quite unnecessary and in many others far too high. Such expenditures would, he contends, amount in all to not less than £1,600,000 for the Russian railways generally.

In spite of this unsatisfactory condition there are some very considerable extensions partly in course of construction, partly proposed, most of these being in the southeastern districts of European Russia, for the purpose of connecting the rich mineral districts of the Ural Mountains as well as the large corn-growing districts with the centers of industry and the places of export. Woronetz is to be connected with Kharkov, which will bring it in direct communication with Nicolaiev and Odessa. A deputation has again waited upon the Government petitioning for the building of the line from Nijni Novgorod, by Kazan, Menselinsk, Ekaterinburg to Tyumen, a distance of some 800 miles intersecting the Ural Mountain range at Ekaterinburg, and so far forming the first link in the chain of proposed overland connection to China. Another line from Samora, a station on the Syzrau Orenburg line is to lead over Bogoroslay and Ufa to Ekaterinburg. This latter railway, which will have a length of about 450 miles, would, in conjunction with the former, open up a very large and productive district principally to the Black Sea ports and Western Russia, which is now almost cut off from commerce on account of the very slow and expensive means of communication, all traffic being carried on by vehicles at about five times the rate of the highest railway tariff in Russia. Some 70 miles of the trans-Caspian railway are already finished, and other new lines are under consideration.

The Government intend to commence two new lines, the Krivoi-Roy railway and the Baskentshak line this year, they are together about 400 miles long, and are to be built en-

tirely at Government expense. The cost of these lines, which are to be carried out under the superintennence of Mr. Titow, late chief engineer of the Donez Railway, is calculated at £16,130 per mile; one of the main objects in building them at the present moment is to give employment to a large and almost destitute country population. Great difficulties are anticipated with the unskilled laborers, and there can be no doubt that agricultural laborers and carters, of which this population principally consists, are but indifferent raw material for railway work. One of the largest iron structures on the Krivoi-Roy line will be a bridge over the Dnieper near Ekaterinoslav.

IRON AND STEEL NOTES

INCREASING THE USE OF IRON.—The Belgian Commission of Inquiry into Means for Increasing the Use of Iron, which was appointed in 1877, has lately issued its report. For the better consideration of the subject it has been divided into seven classes, namely, engineering construction, buildings, mining, railway stores and fixed plant, railway rolling stock, naval construction, military material and buildings. Almost the only suggestion which is made under the first heading, is that of substituting iron columns for stone piers in bridge building; whilst, in place of wood, iron screw piles are recommended for pile foundations, and iron for bridge roadways, as being both stronger and cheaper than wood. Reference is made to the employment of iron in marine piers, wharves, &c. The report urges its use in buildings wherever possible, especially in all those liable to fire, in which latter case iron floors, staircases, girders, &c., should be obligatory. Doors, shutters, roofing laths, &c., might also be constructed of this material.

For the lining of pit shafts iron is considered by the Commission to possess advantages, as in this manner the dimensions of the shaft would be reduced. In some cases it might replace wood in the permanent galleries; and there are many other purposes for which iron might be employed in mining work, to the exclusion of wood, and the obviation of its many disadvantages.

On the question of iron *versus* wooden sleepers, the report refers to the increasing preference given to the former, especially in Germany; but the Commission appears not to have had sufficient information before it to arrive at a decision. The Minister of Public Works has been requested to furnish a report on the subject.

On the question of naval construction, the opinion expressed is, that for vessels under 500 or 600 tons, wood is the cheaper material; whilst, as larger vessels are at the present time almost exclusively constructed of iron, no extension of its employment in this direction is suggested. For military purposes iron might be much more largely used than at present.

Practically, it would appear that the Commission offers scarcely any original suggestions for the employment of iron. Many of the points mentioned in the report have already been, to a great extent, successfully adopted; whilst on the most important point touched upon—that of iron *versus* wooden sleepers—the Commission discreetly refrains from expressing an opinion. Taken as a whole, it must be admitted (from the report) that its work is hardly likely to produce any practical result; but it may possibly lead to a more thorough consideration of the question, and thus attain some measure of usefulness.

STEEL PLATES IN RUSSIA.—According to the regulations now in force at the Russian Government yards the steel plates there used for shipbuilding or boiler making purposes are to be rolled from ingots containing from 0.18 to 0 22 per cent. of carbon, and in the case of the plates for boiler making the test samples have to stand a tensile strain of not less than 26 or more than 30 tons per square inch, and must elongate 20 per cent. in a length of 8 in. before fracture. Shipbuilding plates must have a breaking strain of between 26 and 31 tons per square inch, and must elongate not less than 17 per cent. in a length of 8 in. The hot and cold bending tests are the same as those of the English Admiralty for iron, but in the case of the cold bending tests the samples are to be placed for from 20 to 30 minutes in a cooling mixture giving a temperature of about zero Fahrenheit, and after this they must bend to the same angle as is required for iron at the ordinary temperature.

THE HARDENING OF STEEL.—The tempering of steel is a question which is attracting considerable attention at the present time, especially the relation between the metal and the gases which come into contact with it during the process of manufacture. An interesting communication on the subject was recently made to the Physical Society by Professor Chandler Roberts of the Royal School of Mines, and his principal result, though of a negative kind, is valuable as narrowing the question at issue. Professor Roberts began by tracing the history of our knowledge concerning the carburization of iron, from the work of Clonet at the end of last century to that of Margueritte in 1856. Margueritte showed that, although the conversion of iron into steel could be effected by contact with carbon even in the diamond form, it is nevertheless true that carbonic oxide ordinarily plays a considerable part in the process. Graham's paper "On the Occlusion of Gases," read in 1867, gave singular point to this conclusion by showing that carbonic oxide can penetrate to the center of a mass of iron. This gas is in fact introduced into the iron at a comparatively low temperature, while a high temperature is necessary to enable the metal to appropriate the carbon in order to become steel. The effect of occluded gases in iron and steel is now being carefully studied by metallurgists in general, and a committee of the Institution of Mechanical Engineers recently raised the question in one of their reports as to whether the hardening and tempering of iron and steel might not be pro-

duced by the expulsion of occluded gases during the heating process, and their subsequent exclusion by the sudden cooling and contraction. Professor Roberts has undertaken to answer this question, and by heating rods and spiral wires of steel *in vacuo*, by means of the electric current, and suddenly quenching them in cold mercury, he demonstrates that steel will harden when there are no gases to absorb. The metal was of course robbed of its occluded gases by means of an air pump connected to the vacuum chamber, and the parts which were quenched in the mercury were found to be glass hard, while those which did not reach the cold fluid were found to be quite soft. Professor Roberts therefore concluded that gases do not play any part in the process of hardening and tempering. Historically interesting are the facts mentioned by Professor Roberts, that as early as 1781 Bergman clearly stated that fixed air could give up its carbon to iron, and that Reaumur, in 1722, actually employed the Toricellian vacuum in experiments on the tempering of steel, the metal being placed red hot in a highly rarefied atmosphere, thereby anticipating the methods of to-day by more than 150 years. An interesting discussion followed the reading of the paper. Professor Hughes, who has made numerous experiments on the subject, expressed his opinion that the temper of steel was due to the chemical union of the iron with the carbon. At low temperatures this union takes place only in a slight degree, and hence in soft steel we have the carbon keeping aloof from the iron; but as the temperature is raised the combination is further thered, until in the case of grey or glass hard steel we have really a kind of diamond alloyed with iron. Sudden cooling is necessary to fix the combination, for in slow cooling the carbon separates out again from the iron. This theory is a very promising one, and is supported by a variety of facts; Mr. Stroh, for example, having observed that when an electric spark passes between two iron contact pieces and fuses them, the fused part becomes diamond-hard and will scratch a file. Recent researches by Mr. T. W. Hogg have also led him to a similar conclusion, namely, the temper of steel is due to the presence of an unstable compound of iron and carbon. The theory might very well be tested by chemical analysis in order to see whether the proportion of carbon appropriated by the metal increased with the temperature, or if any change took place in the refractive index of the steel. It was generally agreed by all the speakers at the meeting that the color of the surface of tempered steel depends on the temperature, and is due to the thickness of the film or skin of oxide; the blue film signifying a higher temperature than the yellow, as well as a thicker coating. In this connection Professor Hughes has demonstrated that the electric resistance of the film increases with the temperature. A novel illustration of metallic skins was furnished by Professor Guthrie, who exhibited a steel chain to which he had given a beautiful bluish-black protective coating by simply dipping it in melted nitrate of potash or common nitre. The process was discovered accidentally, and as the bloom improves the appearance of the metal, it will probably be applied to utensils of iron and fancy articles.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

MONTHLY WEATHER REPORT FOR JUNE. Washington: Government Office.

EXPONENT OF THE PRINCIPLES OF MOMENTS. By W. S. Auchincloss, A.S.C.E. Reprint from Transactions of A.S.C.E.

ABSTRACTS OF PAPERS IN FOREIGN TRANSACTIONS AND PERIODICALS. Vol. 64, Part 2. London: Published by Institution of Civil Engineers.

THE THREE AMERICAS RAILWAY.—By Several Authors. Saint Louis: W. S. Bryan.

The Editor of this collection of Essays is Mr. Hinton Rowan Helper. The writers of the principal Essays, five in number, are Messrs. Frank F. Hilder, Frederick A. Bulen, Wm. W. Archer, Frank De Yeaux Carpenter and Francis A. Deekens.

The proposed route for a railway which is intended to bind together North America, Central and South America, is, as set forth, by Mr. Helper: "I have a decided preference for a perfectly direct track all along down the twenty-eighth degree of longitude west from Washington (103° west from Greenwich) from the southern boundary of British America to the northern frontier of Mexico. The line thus indicated, from which I would only with reluctance deviate so much as a hair breadth, either to the right hand or the left, but would follow with such exactitude that the roadbed from the extreme north to the extreme south of our own Republic, would be as rectilineal as the straightest street in the most quadrangular city, intersects Dakota, Nebraska, Colorado, New Mexico and the western part of Texas. Thence in a perfectly straight line to the City of Mexico, then, as nearly midland as might be found advisable, through Guatemala and other Central American Republics to the Isthmus of Darien, and thence into South America, passing eastward of the Andes, felling the forests and furrowing up the surfaces of Columbia, Ecuador, Eastern Peru and Bolivia; thence in a nearly straight line to Rosario, Buenos Ayres and such other points in the Argentine Republic as the present needs of postal facilities and trade and travel may demand."

If anything connected with this scheme could be more striking than its political features, we are inclined to think the engineering ones will be when the road is fairly constructed on the proposed route.

EXPERIMENTAL ORGANIC CHEMISTRY. By Professor Chapman Jones. (London, Joseph Hughes.) New York: D. Van Nostrand.

This much neglected though most important branch of chemistry is, we are glad to see, at last receiving a little attention. It would be difficult to find language which would rightly

decribe the process now in vogue by which organic chemistry is supposed to be taught. After a course of lectures and a term spent over mineral chemistry, the student is set to prepare one or two organic re-agents that happen o be in demand, and his training is complete. The little book before us will, we think, do something towards putting an end to so absurd a method of teaching, and will at any rate supply a trusty and efficient basis for the student. Although not a pretentious book, it is, as the author says in his preface, practical and reasonable, the student being taught to observe and compare, instead of being asked to swallow isolated facts. The work is altogether free from fantastic theories, and we notice several pages that will be read with great interest by earnest students, who will be surprised to find that old friends can show such new faces. We especially notice the remarks on the definition of organic acids (pp. 60, 61), as being to the point and in advance of the notions promulgated in existing text books. To science teachers and candidates preparing for the Indian Civil Service this book will be found extremely useful.

TEXT-BOOK OF SYSTEMATIC MINERALOGY. By Hilary Bauerman. Longmans, Green & Co. 1881.

A systematic mineralogy has long been much wanted, which would occupy an intermediate position between the small elementary text-books which confine themselves to giving briefly and in a more or less disconnected form a general description of minerals, and the large works which partake more of the nature of a dictionary. The author of the work under consideration has endeavored to make the book connected and systematic throughout. Even if he had not succeeded, credit would be due to him for the attempt. It is extremely difficult in compiling a small text-book so to arrange that every part of the subject shall receive attention in exact proportion to its merits. In the endeavor to accomplish this, and at the same time to avoid giving meagre descriptions of important things, the author has been forced to consign descriptive mineralogy to another volume, not yet issued. It is true this would not have been necessary had a greater amount of knowledge on the part of the student been assumed. In this, however, we think the author has acted wisely. Instead of stating bare facts, he has prefaced them with a brief description of the principles on which the various phenomena depend. Thus, in treating of the optical properties of minerals, the various theories concerned, such as that of wave motion, are first elucidated. Whether these explanations of points relating to physics will be sufficient for a student without previous knowledge of the subject, is extremely doubtful; but even if they are not, they will materially assist him in obtaining the necessary information by marking out its nature and boundaries. The same thing may be said of the chapter relating to mineralogical chemistry. A student would be very unwise to attempt to obtain from this work alone the knowledge of chemistry requisite to any one studying mineralogy; but he may get useful hints as to the points to which his attention should be more especially directed.

About 200 pages of this book, containing in all about 390 pages, are devoted to physical crystallography. The methods here followed are mainly derived from Groth's treatise. The order adopted has, however, been reversed, and the geometrical properties of crystals are considered before their physical structure. The text is very free from printer's errors. We notice "dihexagonal" for tetragonal. The publishers are certainly to be complimented on their part of the work, and on having introduced to the public a book which will prove very useful to many. Those who would study mineralogy scientifically will find in this volume what is wanting in others which, in respect to price, are within the reach of those for whom this series of text-books was designed.

AID BOOK TO ENGINEERING ENTERPRISE ABROAD. By Ewing Matheson, M. Inst. C.E. London: E. and F. N. Spon, Charing-Cross.

Mr. Ewing Matheson, M. Inst. C. E., has brought out a second part of his valuable Aid Book, published in 1878. That book treated of the inception of public works and of the conditions on which success depends. The present part deals with the different modes of contracting, and enters into various particulars relating to the design or choice of machinery and material with a view to affording aid to foreign or colonial transactions. It would be impossible to give the reader anything but a very general idea of a book of nearly 500 closely printed pages, though a glance at a few chapters and the numerous marginal headings, interspersed here and there with engravings, will enable him to form a notion of the thorough manner in which Mr. Matheson has performed his task. The leading chapter on contract and purchase in the engineering trades is one of the most useful in the book. Speaking of prices and the value of preliminary agreements, the author shows how competitive prices ought to be based. The conditions must be the same in all cases, or the purchaser ought to be aware of the differences and have some standard by which to judge.

Purchase for Export, the Establishment of Factories, the Transmission of Power, are titles of other chapters of the book. In the last, steam, water, compressed air, connecting rods, shafting, &c., are considered, and the engineer or contractor will find the marginal headings numerous enough to assist him in making up his mind on almost any point as to the advantages to be derived from one or the other method; the cross-references and diagrams also are of considerable value. The remarks on hydraulic pressure, Armstrong's accumulator, and the proposal to adopt this and the Swiss plan of conducting high-pressure water through the streets for sale as power are interesting and full of useful data. Air compressors, rock drills and many other methods and motors are described, which the contractor or engineer will find convenient for reference. Coal, iron and steel, are also the subjects of another valuable

chapter. Thus we have the various classes and subdivisions of rolled iron described, such as L and T bars and joints, and the competing iron works in Germany, France and Belgium, are noticed. Much useful technical information will be found in this part on prices, tensile strength, and other tests into which it would be tedious and unnecessary to enter here, and the marginal headings give the text all the convenience and facilities of a dictionary. Fallacious brands are pointed out, and the dimensions of rolled bars of the usual sections for roofs and joists are given. It may be as well to remind the reader that L-bars are of more uniform quality and strength than T-bars, as the rolling is more favorable. Steel is also discussed in detail.

Passing a chapter on railway equipment, we come to a useful one on machine tools, forming a complete handbook on this extensive branch of engineering. Cranes—steam, hand and hydralic—are classified, illustrated and described, according to their use and value in construction, and the contractor will find much to interest him in this part; as also in the next on excavating machines, boring tools, rock drills, pile drivers, &c., about which occasional information may be needed. These various appliances are amply illustrated by small engravings in the margin, sufficient to give the contractor a fair idea of the construction. Thus the boring tools in use for proving foundations, worm augers, chisel drills, rock drills are illustrated, and every point about which a contractor or engineer may need to be informed is briefly explained.

The concluding chapters on Portland cement, iron roofs, buildings and lighthouses concern the architect and builder particularly, but we have no space to enter into these sections. The strength and qualities of cement, methods of manufacturing it, the modes of testing it by briquettes, tests by weight and fineness, and the usual specification qualities, mixture with sand, price and other items of permanent value for reference are given. Every variety of iron roof, modes of lighting and construction, the values of galvanized iron, zinc, copper, or other covering materials will be found illustrated by diagrams and their merits discussed, while fireproof buildings, and the conditions best to insure resistance to heat, are not overlooked. We may have something more to say on these chapters; a detailed review of the work, however, is not necessary to convince us of the thoroughness and value of Mr. Matheson's Aid Book for the contractor, engineer and architect, as it embodies every kind of information most likely to be required in contract work abroad, or, indeed, at home.—*Building News.*

MISCELLANEOUS.

COLD STORAGE.—The recent large increase in the quantities of fresh provisions imported from abroad, has necessitated the erection of suitable apparatus for dealing with such produce on its arrival in this country, as it is obvious much loss and inconvenience would arise by suddenly placing entire cargoes of (say) meat on the market at once, as would have to be done were the meat taken from the cooling chambers in the vessels in which it was imported directly into the ordinary temperatures usually obtaining in this country. To meet this daily increasing demand, companies are being formed for the special purpose of carrying on this business, and we lately had the pleasure of seeing such an arrangement, which has just been set to work under the general direction of Mr. Kilbourn, of 5 East India Avenue. The site of the cold storage chambers is most conveniently chosen, being the basement formed by the Southeastern Railway between Upper Thames street and the river, flanked by streets on two sides and having a wharf at the back. Arrangements are also under contemplation for enabling railway wagons to be lowered from the Southeastern line directly into the works. The system employed is by withdrawing the air from the chambers by means of a Beales exhauster capable of dealing with 15,000 cubic feet per hour, driven by a ten-horse engine, and supplied with steam by one of Fowler's locomotive boilers. Provision is made by which any of the eleven chambers can be placed in communication with the exhauster. The cooling of the air is accomplished by bringing it into contact with a sort of surface condenser placed close to the roof and formed of 6 in. cast-iron pipes, previously refrigerated brine being caused to circulate through these coils, underneath which wooden trays are placed to catch the moisture deposited on the pipes. The brine is cooled at present on Tellier's system, by bringing it into contact with pipes containing methylated ether, this ether being compressed in a cylinder driven by a separate engine. A numerous company were invited to witness the inauguration of the process, when the general principles of mechanical refrigeration were briefly explained by Mr. Kilbourn. The undertaking will doubtless be watched with much interest by all concerned in the food supply question. We have to thank Mr. Tallerman and the general manager as well as Mr. Kilbourn, for the information so freely placed at our disposal.—*London Paper.*

THE "LIGHTNING."—Last July a new system of propulsion, the invention of Mr. Thornycroft, which has been applied to that vessel, was inspected. It will be remembered that the Lightning, built in 1876, was the first of the now considerable fleet of torpedo vessels which has been supplied by Mr. John I. Thornycroft & Co., for the English navy, and that, fitted with an ordinary screw abaft the rudder, she attained upon her official trial, which took place in Stokes Bay, a speed of 18.54 knots per hour. The steering, however, on this occasion was not particularly good, the times of turning the circle being 3 min. 50 sec. to starboad and 3 min. 50 sec. to port; and so, with a view to improve the steering power, and, if possible, also the speed of the vessel, the Admiralty commissioned Messrs. Thornycroft & Co. to fit her up with the apparatus which was the subject of the trial last week. The apparatus consists of a propeller of small diameter encased in a cylinder, which carries

at its after-end fixed guide blades arranged in such a way as to throw the water from the propeller directly aft. Thus far it is similar to the propelling arrangement of Mr. Rigg and Mr. Parsons. It differs, however, considerably from all propelling arrangements of this type in having, the boss of the propeller prolonged in the form of a cone through the fixed guide blades to a considerable distance astern of the enveloping cylinder, the object being, by narrowing the area, to increase the velocity of the steam coming from the propeller while passing through the apparatus. In the case of the Lightning this cone is made in three parts —one consisting of the propeller boss, the second of the boss in the center of the guide blades, and the third attached to the rudder. On each side of the enveloping cylinder are fixed wing pieces, so arranged that when the rudder is put hard over, the wing piece on the opposite side closes the aperture leading from the screw on that side and turns the water entirely through the aperture on the opposite side. As the speed of the vessel had been found, on a preliminary trial, to be over 19 knots, and would be further verified by the Admiralty officials before the boat was taken over, it was not thought necessary to include a speed trial at Long Reach in the programme. After leaving Westminster Pier, where the party embarked, the vessel was made to circle in the river, and it was found that the circle to port was made in 1 min. 15 sec., and to starboard in 1 min. 5 sec.; a result which is a great improvement on her speed of turning on the occasion of her official trial. The stem of the boat was then secured to the pier and the stern propelled sideways, so as to make the vessel turn a complete semicircle against wind and tide round the stem as a center. The vessel was then run down to Woolwich and back, so as to afford some of the party an opportunity of having a short run at full speed, the passage through the Pool in going and returning affording many excellent opportunities for testing the extreme handiness of the vessel. Among the other improvements effected in the Lightning by the introduction of the new propelling arrangement may be mentioned the diminution in size of the propeller, the diameter being reduced from 5 feet 6 inches in the case of the original screw to 3 feet in the propeller now fitted—a very small diameter, indeed, seeing that in some of the recent preliminary trials, the power developed by the engines was 460 indicated horse power. Another important feature, and one which ought to be exceedingly valuable to torpedo operations, is the protection afforded to the screw by the tube in which it works and the grating in front from floating wreckage, &c., an improvement which will no doubt be fully appreciated by the Portsmouth officers. The Lightning will now be finished off and returned to Portsmouth, where she will resume her duties as tender to the Vernon Naval Torpedo School.

A NOVEL form of electric lamp for domestic purposes has been devised by Dr. Paget Higgs, an electrician of New York. The source of light, instead of an incandescent loop of platinum wire or carbonized fiber, is in this lamp a minute electric arc, the carbon points being very small, and separated by a distance hardly perceptible, but the tiny spark formed between them emits a light equal to that of forty candles. The most remarkable feature of the apparatus is, however, its adaptation to the use of stored electricity. Following the indications given by several recent investigators, the inventor has succeeded in forming "secondary batteries," filled with some substance which, by voltaic decomposition and subsequent chemical re combination, affords a practicable means of receiving and retaining an electric charge to be drawn upon afterwards in such quantity and at such times as may be desired. This gives a peculiar advantage in lighting ships or railway trains, but even where a continuous current can be had from a dynamo-electric machine, Dr. Higgs finds it advisable to interpose the secondary batteries for the purpose of equalizing and steadying the light. The principal objection to the electric lamps now used is their variability, and the light even of the incandescent wires flickers quite perceptibly with every vibration of the fly-wheel, so it may well be believed that such a means of accumulating energy would be useful.

Dr. Higgs' system has been so far perfected that a company is already formed, and terms are fixed for the introduction of the new light into private houses. The lamps as now arranged depend upon a weak voltaic battery for their supply of electricity. The current from this would be far too feeble to produce the voltaic arc, but the energy produced by its constant action is stored up in what is called the "secondary battery," and drawn thence as wanted; the total amount of energy produced by the small battery in twenty-four hours being quite sufficient, when drawn in a large stream from the secondary reservoir, to give a brilliant light for four or five hours. The carbons are treated chemically in some way, so that their consumption is slow, one pair lasting a week under ordinary use, and they can be replaced in a few seconds. Unlike all other forms of electric lamps hitherto employed which derive their energy from voltaic currents, the Higgs light is said to be very inexpensive, and the published offer of the company is to place the apparatus, with a number of lamps, in the house of any responsible person on a month's trial. At the end of the month, if the cost of chemicals for producing the light, added to the interest on the first cost of the apparatus, is more than would have been the expense of the same light from gas at the rate of seventy-five cents per thousand feet, the company will take back the whole without charge. The batteries are enclosed in a box which can be set on a shelf in any convenient place, and require no more attention than the house batteries now used for many purposes. An application of the same principle has been made for furnishing motive power; and it is said that a street car motor is about to be put into use which can be charged at the stations with electricity sufficient for a three hours' trip.